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ORIGINAL ARTICLES.

A CONTRIBUTION TO THE STUDY OF THE ACTION OF CHLOROFORM.¹

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WE firmly believe that a clear understanding of the physiologic actions of chloroform on both the circulation and the respiration, especially as regards the relations existing between these two functions under the anesthetic, independently of the influence exercised by the drug on the nervous system, is not only of the deepest interest to the physiologist, but also of the greatest practical importance to the clinician. Through failure of which one of these two vital functions, the circulation or the respiration, does chloroform bring about a fatal result? This is the question at issue, a question of absorbing interest.

There are both surgeons and physicians who boast of never having met with a single death under chloroform-anesthesia. Yet, under the keenest observers and the most careful anesthetizers, death has occurred unexpectedly and in an inexplicable manner, even before the operator had an opportunity to use the knife, and in these cases the fatal result could not be ascribed to surgical shock. The statistics so far published regarding the percentage of deaths caused by chloroform, although valuable to a certain extent, are practically unreliable, and for reasons that need not be discussed here from fear of prolixity. It has been stated somewhere that Billroth met with his first death after 12,500 successful chloroformizations. We have the authority of Wood² for saying that according to Lyman the ratio of deaths in chloroform-anesthesia is 1 in 5860, while Richardson puts it at 1 in from 2500 to 3000. Neve³ has reported 3000 chloroform-administrations without a single death, and Henderson⁴ gives 12 deaths as occurring in 17,666 inhalations by the drug. In an exhaustive collection of chloroform-

administrations, Julliard¹ reports 161 deaths in 524,507, or 1 in 3258 anesthetics. Foy² publishes a total of 877,507 chloroform-inhalations, with a mortality of 204, that is 1 in 4301. Valuable statistics have been presented by Gurlt:³ in 94,123 administrations there were 36 deaths, or 1 in 2614; before this he had collected 6 deaths in 22,656 inhalations, or 1 in 3776.⁴ Quite recently, Gurlt⁵ has reported 46 deaths in 133,729 chloroform-administrations.

Be all this as it may, it is certain that the question of how chloroform kills remains practically a mooted one; and this is our chief excuse for trespassing upon the patience of the profession by offering the results of our individual experience in the matter—an experience based on a special research on the lower animals. Our investigation is not perhaps an elaborate one, but the uniformity of the results obtained in our experiments is sufficient, we believe, to justify us in expressing an opinion somewhat, though not wholly, contrary to the one we had previously entertained. We hope our confrères will express their individual experience and views upon the question, especially from clinical observations, and thus contribute to the elucidation of a subject so ably treated experimentally by authorities like Lauder Brunton, Wood, Hare, and others.

Before the report of the first Hyderabad Commission, it had been the general belief among clinical observers that chloroform destroyed life mainly through cardiac failure. The Hyderabad Commission, however, from a series of experiments upon the lower animals, affirmed that the drug killed by an action upon the respiration, a statment that met with considerable opposition. As a consequence, a second Hyderabad Commission, headed by Lawrie and Lauder Brunton, under the auspices of the Nizam's government, reinvestigated the subject, and, with slight differences, arrived practically at the same conclusions—that is, that chloroform always arrested the respiration before the heart.⁶ This almost unqualified statement led Wood and Hare⁷ to restudy the matter, and from experiments made by administering the drug either by inhalation or intravenously, it

¹ Presented to the Section on Surgery of the Texas State Medical Association, at Austin, April, 1894.

² THE MEDICAL NEWS, August 9, 1890.

³ British Medical Journal, February 8, 1890.

⁴ Glasgow Medical Journal, November, 1890.

¹ Medical Press and Circular, May 27, 1891.

² Ibid., June 3, 1891.

³ Centralbl. für Chirurgie, August 13, 1892.

⁴ Archiv für klinische Chirurgie, 1891.

⁵ Revue de Chirurgie, November, 1893.

⁶ Lancet, London, January 18, 1890.

⁷ THE MEDICAL NEWS, February 22, 1890.

was found that the anesthetic had a direct, distinct, and paralyzing influence on both the circulation and the respiration; that the respiration might cease before the heart, or that both cardiac action and the respiration might stop together; and, finally, that in some instances the heart was arrested before the respiration, the latter at times continuing as long as one and even two minutes after the blood-pressure had reached the abscissa-line.

Cushny,¹ from the results of another experimental research, could not agree with the Hyderabad Commission, that the heart always continued to beat after respiration ceased, and found it very difficult to maintain the concentration of the chloroform necessary to paralyze the heart simultaneously with the respiration, believing from this that such a simultaneous arrest can never occur. A similar view is entertained by Buxton,² an eminent authority on anesthesia, who believes that in the human subject at least death takes place during chloroformization from cardiac paralysis.

In his address on "Anesthesia" before the Tenth International Medical Congress, held at Berlin, Wood³ reported another series of experiments made by himself, and again drew the conclusion that chloroform is capable of causing death by either primarily arresting the respiration or by primarily stopping the heart, but that commonly both the cardiac and respiratory functions are abolished at or about the same time. An identical opinion is expressed by McWilliam.⁴ This investigator concludes, from his experiments, that though the respiration generally ceases before the heart, cardiac failure sometimes occurs long before the respiration is arrested.

The latest research upon this subject of which we are aware is the very elaborate one of Hare and Thornton,⁵ made under the auspices of the Nizam's government. These investigators believe that the question can be settled by the acceptance of both views in a modified form; or, in other words, that there is no real antagonism in the belief that chloroform kills by depression of the heart or by depression of the respiration. They very positively assert that chloroform practically always kills by failure of the respiration when administered by inhalation, provided (and this they consider a most important provision) that the heart of the anesthetized is healthy and has not been rendered functionally incompetent by fright or violent struggles; or, again, by marked asphyxia. And as positively as they assert that the drug destroys life by primarily caus-

ing failure of the respiration, they also assert that in excessive doses by inhalation chloroform has a depressant effect on the circulation, which is chiefly due to centric vasomotor depression, with final depression of the cardiac muscle itself.

Before such a mass of contradictory experimental evidence, what are we to believe? We might venture to say by way of preface to our own conclusions in the matter, that, in the face of the discordant results obtained in the laboratory so far, every medical man, in administering chloroform to the human subject, should follow his own good judgment and study carefully each individual case. It is disastrous in many instances to blindly follow, by a sort of routine practice, the teaching of this or that authority. But we shall discuss this point later.

During the past winter we have conducted a series of experiments exclusively on dogs, and without paying particular attention to the manner by which the various phenomena on the circulation and respiration are brought about under the action of chloroform (a subject that has already been investigated by a host of able experimenters). It has been our chief aim to study how death is produced by the drug primarily, whether by failure of the respiration or by cardiac paralysis. The anesthetic was administered by *inhalation* exclusively, in a concentrated form, through the *nostrils*. For this purpose a common towel-cone was used, and both the respiratory movements and the circulatory changes were registered at the same time upon the revolving drum. We confess that we began our experiments somewhat prejudiced, not to say entirely so, in favor of death by primary cardiac paralysis through the action of chloroform, but the results obtained told a different story. For the sake of illustration we detail the following experiments:

For Experiment I a small dog was used. At the beginning of the inhalation the blood-pressure was 156 mm.; the heart showed a rate of 192 per minute and the respiration 36 per minute. One minute after the inhalation had been instituted, the respiratory movements jumped to 60 per minute, and both the pressure and the rate of the pulse were reduced. The number of respirations became 90 per minute a minute and a half later, and the pulse-rate and the arterial pressure continued still lower. In five minutes more, although the respiration was irregular, the movements marked by the needle were 72 per minute; at this point the pressure was found to be 110 mm., while the heart was still beating 144 times per minute. Gradually the respiration then became full, deep, and slow, and four minutes later only twelve movements were registered upon the drum; three minutes afterward the animal was breathing four times a minute only, the blood-pressure

¹ Lancet, London, March 14, 1891.

² Ibid., December 20, 1890.

³ THE MEDICAL NEWS, August 9, 1890.

⁴ British Medical Journal, October 11, 1890.

⁵ Therapeutic Gazette, October 16, 1893.

EXPERIMENT I.—SMALL DOG.

Continuous inhalation. Carotid artery used for connection with manometer. Respiratory tambour connected with trachea by means of a rubber tube.

Time, m. s.	Pressure, mm.	Pulse, p. min.	Respiration, p. min.	Remarks.
0	156	192	36	
1.00	148	184	60	
2.30	136	180	90	
7.30	120	156	72	Respiration irregular.
9.30	110	144	54	Respiration irregular but deep.
10.30	90	128	30	Respiration full and deep.
13.30	80	108	12	Pulse-waves somewhat larger than at first.
16.00	62	92	4	
16.30	50	86	...	Respiration ceased; blood-pressure going down steadily.
17.00	32	40	...	Pulse very small.
18.30	10	68	...	Pulse continues very small.
19.00	Heart still beating about once or twice a minute.
20.00	Applied artificial respiration for about three minutes, by compressing chest, when both the respiration and the heart's action were brought back. Under another inhalation of the drug, death ensued, the breathing, as before, stopping first.

EXPERIMENT II.—DOG, WEIGHT 10 LBS.

Carotid artery connected with manometer.

Time, m. s.	Pressure, mm.	Pulse, p. min.	Respiration, p. min.	Remarks.
0	140	210	30	
2.40	138	212	30	Inhalation begun.
2.30	90	132	60	
3.30	80	108	48	
5.40	50	92	18	
6.00	32	80	2	Pulse-waves somewhat larger than normally.
6.30	12	30	...	Breathing ceased.
7.40	8	48	...	
8.00	8	62	...	Pulse very small.
8.15	Heart stopped in diastole; remained irritable; blood very dark.

EXPERIMENT VI.—DOG, WEIGHT ABOUT 15 LBS.

Carotid artery connected with kymograph.

Time, m. s.	Pressure, mm.	Pulse, p. min.	Respiration, p. min.	Remarks.
0	120	142	32	
1.00	118	126	36	Began inhalation.
1.40	80	112	42	
2.30	60	90	44	
3.00	52	84	42	Stopped chloroform.
6.00	110	126	42	Resumed chloroform-inhalation.
6.30	100	120	40	
8.00	70	98	40	
8.30	60	80	32	
9.40	32	92	16	
10.00	20	80	8	
12.00	Both respiration and heart stopped simultaneously. Artificial respiration had no effect.

EXPERIMENT VII.—DOG, WEIGHT ABOUT 18 LBS.

Femoral artery connected with kymograph. Continued inhalation of chloroform.

Time, m. s.	Pressure, mm.	Pulse, p. min.	Respiration, p. min.	Remarks.
0	140	180	30	
0.15	110	150	30	
1.00	56	96	36	
4.00	40	90	36	Respirations deep.
5.00	32	78	20	
6.30	20	38	8	
9.30	20	36	...	Respiration suddenly stopped.
10.30	10	24	...	
11.30	Heart still beating about four or six times a minute; it soon ceased. Opened chest; heart was found in diastole, full of black blood, and irritable to mechanical stimulation. On making disconnection, very dark blood continued to flow from femoral artery, the color of the fluid being evidently due to the asphyxia produced.

being 62 mm., and the rapidity of the cardiac beat 92 per minute. In half a minute more the breathing stopped, and then the rate of the pulse, with slight variations evidently due to cardiac weakness, and the arterial pressure continued to sink, and in two minutes afterward the needle of the manometer stood still at the abscissa-line; but it was noticed that about once or twice a minute a curve could be registered upon the cylinder, the result of cardiac activity. When the animal was thus dead to all intents and purposes, the chloroform was removed and artificial respiration applied. This was done for about three minutes, by manual rhythmic compression of the chest, the tongue at the same time being pulled out with a hook; and following this operation both the respiration and the heart's action were brought back; in other words, the animal was resuscitated. After a sufficient time, when the two vital functions may be said to have been thoroughly reestablished, inhalation of chloroform was again applied, and similar phenomena to those described were observed, death taking place from failure of the respiration.

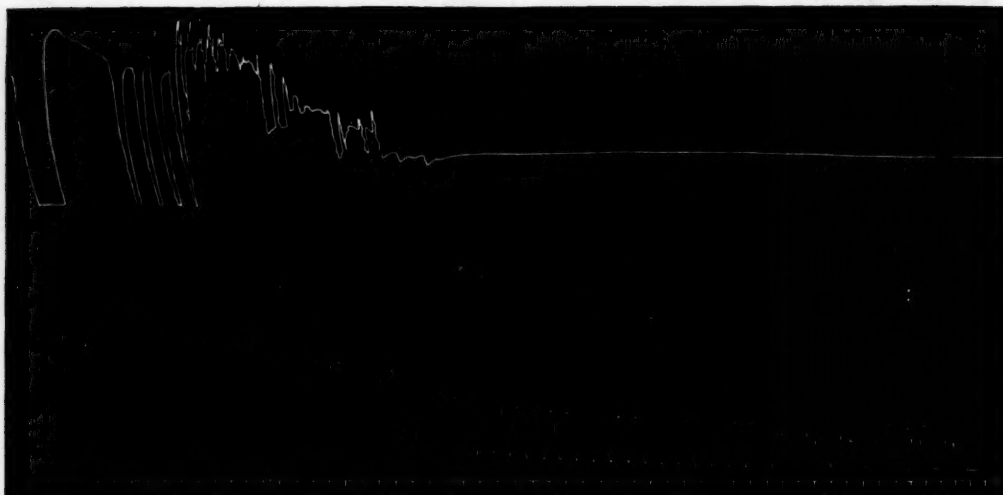
Identical phenomena were noticed in Experiment II, but (although the animal was larger and weighed more than that of Experiment I) the deleterious effects appeared sooner. In six minutes after the beginning of the inhalation, the blood-pressure was 32 mm. (originally 140 mm.) and the cardiac rate 80 per minute (originally 210 per minute). The respiratory rate, which from 30 had been raised to 60—that is, had been doubled shortly after the commencement of the chloroformization—was by this time depressed to 2 per minute, and half a minute afterward the respiration ceased, the heart continuing to beat for about one and three-quarters of a minute longer. The chest of the animal was immediately

opened, and it was found that the heart had stopped in diastole, its cavities being full of dark blood and still responding to mechanical irritation.

Experiments III, IV, and V gave exactly the same results. The respiration in each case ceased before the action of the heart.

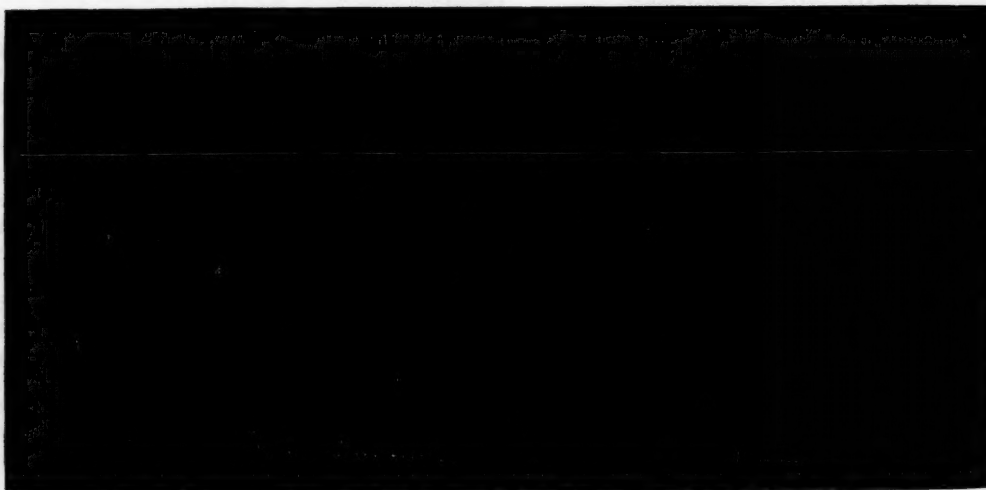
however, that the inhalation of chloroform was purposely interrupted at a period when the circulation had been considerably depressed. The withdrawal of the vapor was followed by a decided tendency of the blood-pressure and the heart's action to assume their original condition; but before this was com-

FIG. 1.



Tracing showing how chloroform causes death by respiratory paralysis. Upper line represents respiratory movements; middle line, blood-pressure and pulse; lower line, abscissa and time in seconds.

FIG. 2.

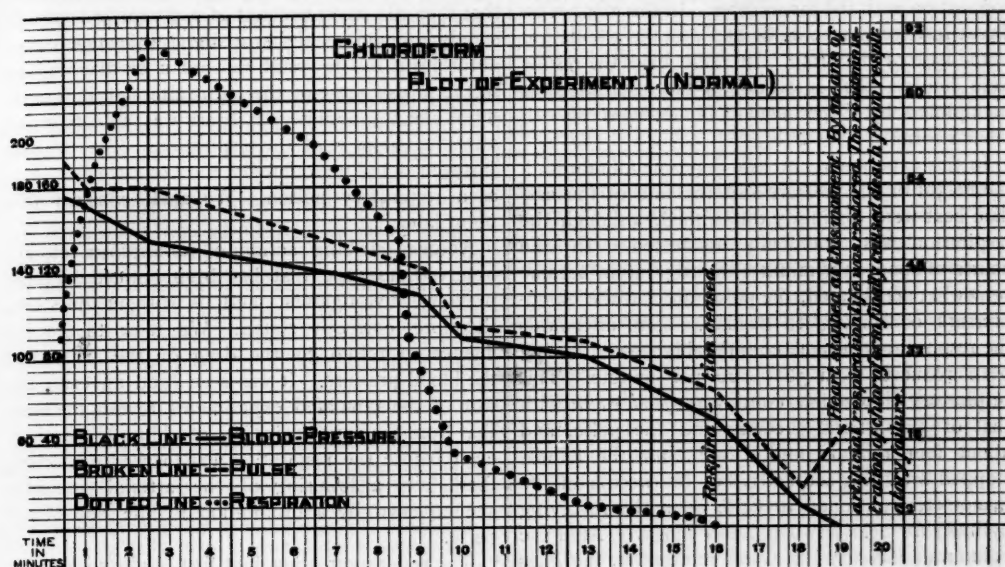


The same tracing without interruption. It will be seen that the heart ceased one minute and a half *after* the respiration.

A dog weighing about fifteen pounds was used for Experiment VI. In this instance the results were different—that is, simultaneous arrest of the heart and the respiration was observed. It may be stated,

pletely established, the inhalation of the anesthetic was resumed and the changes produced closely observed. It was seen that both pulse-rate and arterial pressure went down *pari passu* with the depression

FIG. 3.



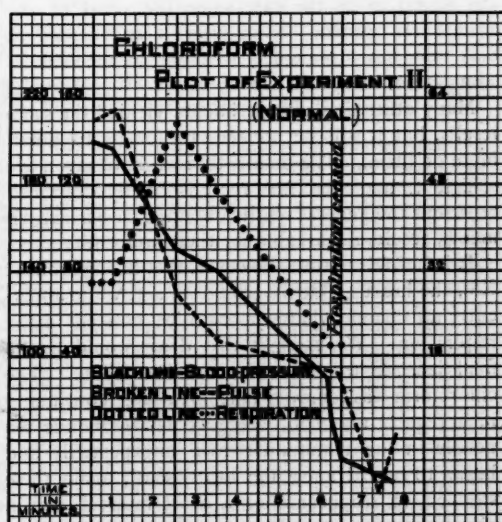
of the respiration. Six minutes after the beginning of this second inhalation, both needles, that of the tambourine and that of the manometer, came to a standstill *simultaneously*. Artificial respiration was then applied vigorously, according to the method described, but all our efforts in this direction were of no avail. Although there appeared some reflex respiratory movements and an occasional heart-beat, life could not be restored.

For Experiment VII a medium-sized dog was used. The experiment was performed before Professors William Keiller and James E. Thompson, of the University of Texas, and in the presence of the medical students of the third-year class, Professor Keiller himself administering the chloroform. The same results were produced. Following the inhalation of the drug, a lowering of the arterial pressure and of the rate of the pulse was observed. The respiration was, as usual, slightly increased in rate at first, becoming depressed soon afterward. A lowering of the blood-pressure and of the cardiac and respiratory rates was then progressive until the occurrence of death, this taking place distinctly and directly through failure of the respiration, the heart continuing to beat for about a minute and a half longer.

One of our experiments (No. V) was also witnessed by Dr. H. C. Cooke, of Galveston (and likewise of the University of Texas), who, though convinced of the accuracy of the experiment and the, to him, unexpected result of death from respiratory failure, still maintained that interference with the circulation at the base of the brain by tying the carotid

artery might have some influence in enhancing, at least, the death of the respiratory before that of the cardiac centers. This, we contended, had little

FIG. 4.



or nothing to do with the result, as in the experiments of other investigators, in which the carotid artery had been frequently used for the kymograph-connection, death is reported to have occurred also primarily through cardiac paralysis. To further satisfy ourselves of this, however, and to put aside objections such as that offered by Dr. Cooke, we

made experiments by using the femoral artery for connection with the manometer. In this manner no direct interference could be had with the circulation at the base of the brain. In these cases the results were similar—that is, failure of the respiration, as exemplified by Experiment VII, detailed also in tabular form.

In this experiment, in less than one minute both the height of the pressure and the rate of the cardiac beat were distinctly diminished, although the respiration was practically unaffected. In the course of about four minutes the respiration became somewhat accelerated, the pulse-rate and the blood-pressure continuing to decline. Soon afterwards the respirations became considerably slower, having been reduced from 36 to 20 per minute. This respiratory depression continued *pari passu* with that of the arterial pressure and the pulse, and about four minutes and a half later the animal stopped breathing suddenly, but the needle of the manometer continued to write upon the revolving cylinder for nearly two minutes longer. After the column of mercury had stood motionless, the glass canula was immediately separated from the femoral artery; very dark blood continued to flow from that vessel, the color of the fluid being due undoubtedly to asphyxia; its ebbing (the animal continued to lie in a perfectly horizontal posture) showing that the heart was still working. And this was found to be the case when the chest of the animal was opened. Even after it had ceased to beat, the heart remained irritable to mechanical stimulation.

Many other similar experiments yielded the same results, but it will be unnecessary for us to detail more of them. The details that we have given are sufficient, and the few records briefly described represent the general results obtained in our experiments, results remarkable for their uniformity.

It is but fair to state, however, that in Experiment XV, the heart suddenly ceased to act, while apparently no change whatever could be noticed in the respiration. We thought that death would be the final issue in this case from cardiac failure. The chloroform was removed (the breathing continuing in a regular manner), when in a few seconds the needle in the manometer rose unexpectedly and began to record anew the heart-beats; again these stopped, to reappear after a short interval, and in about three minutes the respiration ceased, and in spite of this the heart continued to act for a few seconds longer, then finally coming to a standstill. How could this phenomenon be interpreted?—a cardiac or a respiratory failure? It was not a true respiratory failure, because even when the circulatory disturbance was marked, the respiration was apparently unaffected; and if the respiratory centers

succumbed finally, it may properly be said, perhaps, that they did so on account of a diminished blood-supply and not owing to a direct influence of the chloroform. The result was not a cardiac failure in the proper sense of the term (although it looked more like it), because the heart was able to work after the stoppage of the respiration. The drug in this case acted undoubtedly more as a cardiac than as a respiratory poison. We shall consider this point later.

Another striking action of chloroform closely observed was one that has been noticed by most previous investigators, and is without question, therefore, correctly stated. We refer to the influence exercised by the drug on the pulse and the blood-pressure. It was seen (and an examination of the tabulated experiments reveals the fact) that from the very beginning of chloroformization, even before the respiration was markedly affected, there was a lowering of the arterial pressure, accompanied by a diminution in the rate of the pulse.

The respiration, though at first slightly increased in rate, became gradually depressed and generally and, we may say, practically always stopped before the heart. In not a single experiment did we see distinct cardiac failure preceding the stoppage of the respiratory function, and in only one case surely, and in a second one probably, was there noticed a simultaneous arrest of both the respiration and the action of the heart.

The general and marked results of our investigation lead us to conclude that chloroform given by inhalation and in excessive amounts, other things being equal, destroys life mainly through failure of the respiration. It is true also that during chloroformization there is considerable circulatory depression, and this latter phenomenon may likewise become exceedingly dangerous. While we, therefore, agree in the main with the results of the second Hyderabad Commission and with the similar ones obtained by Hare and Thornton, in that death from chloroform administered by inhalation to the healthy organism is due primarily to a direct action on the respiratory centers, yet we do not believe that during chloroform-anesthesia in the human being, no matter how favorable the circumstances, the respiration alone should be watched. While the disappearance of the radial pulse, for instance, is not an absolute sign that the heart has stopped beating, it is, nevertheless, a very positive indication that the circulation is much depressed. Now, it is possible for this phenomenon to be present—that is, a weakened condition of the circulatory apparatus may be so marked as to render it difficult to detect a radial pulse—and yet the respiration may continue to be apparently normal. Are we, under such

circumstances, and from the fact that the respiration has not yet shown any signs of disturbance, justified in continuing the administration of the chloroform?

Even though experimental evidence has apparently demonstrated that, under chloroformization, there is more danger from respiratory than from cardiac disturbance, it becomes our duty, it seems to us, to remove our patients not only from the main source, but from all possible sources of danger. A weakened circulation brought about by chloroformization is a source of danger, undoubtedly, and it becomes evident, therefore, that that function should not be neglected simply because we have more cause to fear a failure of the respiratory function.

Hare and Thornton state that

"As there is always a fall in pressure under chloroform, it is difficult to feel the radial or temporal pulse, and the respiratory center recognizes the degree of arterial depression which its sister vasomotor center has permitted by finding that its blood-supply is insufficient. As respiration fails first, it should be watched first. Finally, it is only by watching the respiration that we can tell how much chloroform the patient is getting. We do not watch this function for danger alone, but to tell us of the dose."

The same investigators have remarked:

"That the circulatory depression may be dangerous is not only evident, but it is stated to be so by the second Hyderabad Commission itself at the end of paragraph 8. This circulatory depression may be so profound that recovery is impossible, even with the most thorough artificial respiration, a fact stated by the second Hyderabad Commission in paragraph 25.¹ . . . This emphasizes the fact that *we cannot afford to totally ignore the effect of chloroform on the circulation,*² and we cannot consider the patient in danger of circulatory failure *only* when the respiration ceases, BUT AS SOON AS IT BECOMES ABNORMAL."

We do not entirely agree with the last part of the foregoing statements. We hold that the circulation, independently of respiratory disturbances, may become abnormal also, either through cardiac weakness or vasomotor changes, or both, and, therefore, dangerous; and we further hold that the circulation, in the large majority of cases, *becomes abnormal as soon as it becomes depressed*. In some cases this abnormality is made manifest before there is any disturbance of the respiration. The result obtained in Experiment XV is very significant, and we firmly believe that parallel instances are of more frequent occurrence in the human being, under the full influence of chloroform, than is generally supposed. Therefore, if we are to watch the respiration in every case, as advised by Lawrie, irrespective of the pulse, there is no doubt that we shall continue to increase, instead of diminishing, the death-rate in chloroform-administration.

Hare and Thornton again remark:

"On the other hand, we should remember that, even if chloroform has been given properly, the arterial pressure may be so low as to give no pulse in the radial artery, and yet the circulatory system be ready to respond at once when the drug is removed. If, therefore, the chloroform is properly administered, is there danger of its circulatory effect in man?"

—implying apparently by these expressions that, under such circumstances, there is little or no danger from a depressed circulation if "the chloroform is properly administered." But we believe that a depressed circulation, independently of any respiratory disturbances, should put us always on our guard; and for greater reasons should such a depressed circulation, particularly when carried to the extent of causing a disappearance of the radial or temporal pulse, be a sufficient warning for us to remove the anesthetic at once. Because, even granting that more often than not the circulatory system, depressed by chloroform, is ready "to respond at once when the drug is removed," we cannot tell how long we may with absolute safety prolong the administration of the agent after the appearance of the circulatory depression.

As much as we are now convinced that chloroform, administered by inhalation, produces death in the lower animals (for example, dogs) chiefly by primarily arresting the respiration, we are also satisfied from clinical observation and from the results of our experimental research, that *both the circulatory and respiratory functions should be watched at the same time*. We do not, therefore, entirely agree with this statement of Hare and Thornton:

"While watching the respiration will not warn us of a sudden cardiac arrest in fatty heart plus chloroform-depression, neither will the pulse give us such a warning; and we are confident that the statement of the Hyderabad Commission, that *the respiration should be watched*, is correct, for we believe, from a long series of observations, that gradual cardiac failure never occurs without producing respiratory changes from the very first. In other words, we do not believe that in a *healthy* heart chloroform can cause serious disorder without, as a result of beginning disorder, disturbing respiration; and, second, that in a healthy heart a quantity of chloroform sufficient to disorder it will by its direct action disorder the respiration.¹ If, as an extra precaution, one assistant watches the pulse, while the other watches the respiration, very well, for though the respiration is the more important function to watch, the man watching the pulse might discover an irregularity which the anesthetizer may not see reproduced in the respiratory action; but as divided attention generally means a slighting of both objects in view, *Lawrie is right in insisting on the pulse being let alone.*"²

We are not justified in letting one function alone simply because its disturbance is not as dangerous as that of the other function. It is our duty by all

¹ We have had a similar experience in many of our experiments.

² The italics are ours.

¹ We do not think this is necessarily the case, especially if we take into consideration constitutional idiosyncrasies, etc.

² The italics are ours.

manner of means to enhance the safety of our patient. It is well enough that in experimentation upon the lower animals, one or the other function, or both, be entirely neglected, *in order to determine which one stops first*; but this sort of experimentation cannot be carried on in the human being. Moreover, though experimentation upon the lower animals is the best means we have at our command for arriving at scientific conclusions, when we come to the human being we cannot proceed entirely from analogy—that is, from the results obtained in the laboratory. Again, even admitting that it has been positively demonstrated that death under chloroform occurs mainly from respiratory failure; and, on the other hand, if it be shown, *as it has been shown*, that there is danger in disturbance of both functions, no matter which one of the two is the more easily affected, it is but fair, it is but reasonable, to insist that *both the circulation and the respiration should be watched at the same time*, and not one function alone. Science and practical experience demand the careful employment of this double precaution.

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A PLEA FOR THE EXCISION OF THE INITIAL LESION.¹

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EXCISION of the initial lesion as an *abortive* measure has been practised at irregular intervals since 1693, when M. Corbis, of Lille, first excised the sore, contending that the poison had not yet entered the system. Since that time some surgeons have followed his lead, but many more have opposed the treatment. Each advocate of excision has had an idea of aborting the disease, but no one, so far as I know, has advocated the procedure as a method of treatment to lessen the secondary symptoms. I hope to show that by early excision of the initial lesion the disease may be aborted, and by excision in the later stages the secondary symptoms can be moderated.

I am very sorry to disagree with any opinion expressed by my friend Dr. R. W. Taylor, but I can hardly accept his explanation of the matter as laid down in a very elaborate paper entitled "Why Syphilis is Not Aborted by the Destruction or Excision of the Initial Lesion."² In this paper two particular cases are referred to, and conclusions are drawn from the patho-

logic condition as demonstrated, but we are not justified in assuming that these conditions existed in an earlier stage of the disease. That would have to be actually demonstrated before an argument could be based on their existence. Again, the condition demonstrated must be constant and have some particular relation to the dispute in question. If the identical condition can be demonstrated in other diseases, then no conclusions of a positive nature, attributed to one disease, are justifiable. Dr. Taylor concludes from the microscopic appearance in these two cases—one of a chancre of about four days', and the other of ten days' existence—that it is owing to the rapid infiltration with leukocytes of the peri-vascular spaces and tissues immediately surrounding the bloodvessels. (Figs. 1 and 2.) I quote from his article:

"The point deserving of attention in the first case is the extremely early and far-extending involvement of the bloodvessels; although the primary sore in the first case is but of a few days' duration and very small, and under the microscope is of such limited and circumscribed extent, the bloodvessels are very extensively surrounded by cell-investment, at a considerable distance from the ulcer. The microscope shows *how very deeply rooted syphilis is at the beginning of the sore by having propagated itself along the peri-vascular lymph-spaces, and how futile it is, as experience has already shown, to attempt to stay syphilis by excising the primary sore. Apparently, judging from the appearance of the vessels in this case, their involvement begins before the appearance of the sore.*"¹

In the foregoing paragraph no consideration is given to inflammatory change, or to the irritating effect from the development of a toxin at the site of the lesion. We know from bacteriologic studies that an infiltration of leukocytes is found in the tissues in about two hours (or less) after an irritating poison has been applied. This infiltration of leukocytes is all that has been demonstrated here. We are asked to look upon these as the poison of syphilis, but we can hardly do that. Some other conditions must be demonstrated; for instance a specific bacillus in this tissue. I have here a section made from a chancroid, removed on the second day after its development, which presents identically the same appearance as that described by Dr. Taylor. (See Fig. 3.) I shall quote one paragraph from his paper that will be an excellent description of my specimen:

"This change in the bloodvessels consists in the distention of the peri-vascular spaces with small, round cells. Nearly every vessel in the section, both artery and vein, is in this way enveloped by masses of small, round cells, forming a sheath, like

¹ Read before the American Association of Genito-urinary Surgeons, at Washington, May 31, 1894.

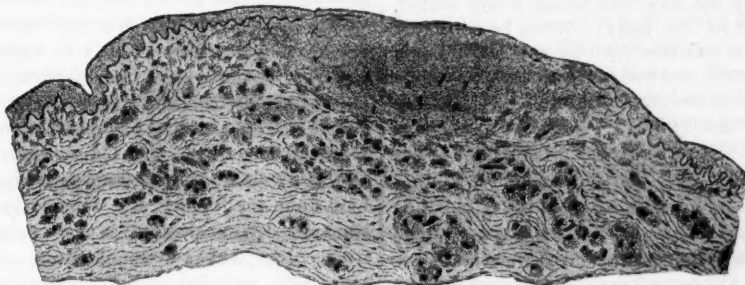
² Medical Record, July 4, 1891.

¹ Italics mine.

a coat-sleeve around the arm. . . . Besides this condition of the peri-vascular spaces there is a change in the endothelial cells lining the arteries

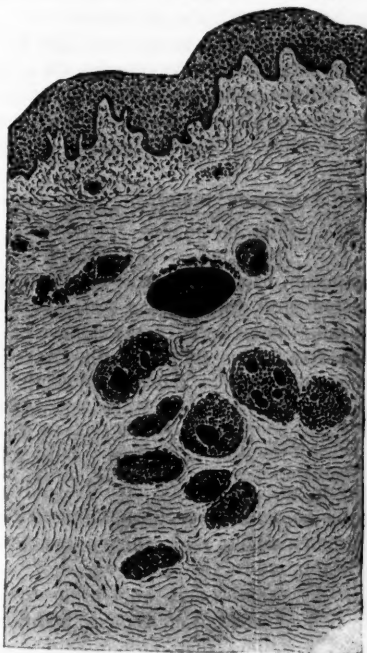
the chancre after the first period of incubation, the poison is deeply rooted beneath the initial lesion, and that it extends far beyond it; that it is in a

FIG. 1.



Showing the chancre (at the right upper part) and small vessels, with the coat-sleeve arrangement of the cell-infiltration in the deep connective tissue under and beyond the chancre. (Vessels represented by black dots.) [Taken from lithograph illustrating Dr. Taylor's article in the *Medical Record*, July 4, 1891.]

FIG. 2.



Showing coat-sleeve arrangement of the cell-infiltration in the skin, far away from the chancre, which to the eye looks healthy. (Vessels represented by black dots.) [Taken from lithograph illustrating Dr. Taylor's article in the *Medical Record*, July 4, 1891.]

and veins. The endothelial cells are swollen, and seem to be proliferating."

I shall quote one more paragraph from Dr. Taylor's article:

"These studies therefore go to show that in the very first days of syphilitic infection, as shown by

FIG. 3.



From a section of chancroid, showing coat-sleeve arrangement around the vessels and general infiltration of the tissue with leukocytes.

most active state, and running along the course of the vessels it soon affects all the parts beyond, even to the root of the penis. These studies seem to warrant the conclusions that the virus is not localized at the point of entry, and that it does not shut itself in by throwing out a dense wall of circumvallation which, later on, disappears and allows the exudation of the morbid products of the heretofore supposed closed-in morbid focus."

Here again the infiltration is pointed to as the syphilitic poison. From the conditions found in and surrounding the chancres, Dr. Taylor concludes that the poison is not localized in the sore. Certainly not, after a certain amount of the poison has entered the circulation, but this localized sore is a reservoir from which new poison is continuously poured into the system until the induration becomes entirely absorbed. The paper really strengthens the case of those who advise excision at any stage.

CASE I.—J. M., twenty-three years old, presented a chancre that developed twenty-two days after intercourse. He was treated by a druggist, who told him the matter was only trivial. The ulcer was very slow in healing. About six weeks after the man noticed the sore, and before it had healed, a rash appeared on his body. When he consulted me the chancre was one inch by one-half inch in size and presented marked induration. The adenitis was excessive and very painful. The rash was general and very profuse. I excised the lesion at once and the wound healed by first intention. The adenitis began to subside within thirty-six hours, and a marked change took place in the rash within seventy-two hours. I now placed the man upon specific treatment; the sequelæ were few and easily controlled. The first improvement I attributed to the removal of the cause of irritation, and the second to the cutting off of the supply of germs.

While the specific microbe of syphilis has not yet been isolated, it is almost beyond doubt that syphilis is caused by a specific germ. Although Lustgarten, Zeissl, Klebs, Doutrelepon, and Schultz, and others have all isolated bacilli, yet none has stood the test of further research, and, therefore, we cannot claim to know their absolute importance as a factor in the causation of the disease. The very long period of incubation of the disease and the peculiarity of soil necessary for the cultivation of the bacillus appear to be the most important difficulties in isolating the organism. In the light of recent researches, however, we are justified in arguing from the standpoint of a specific syphilitic germ.

In all germ-diseases there is a period of incubation of variable length. In a good many the effect of the germs is self-limited, whilst in others it is continuous. In all forms of these diseases it is necessary that the poison shall be administered or planted in a soil suitable for the propagation of the germs. That mild cases of these diseases occur proves either that a smaller dose of the germs was administered or that the soil was not properly suitable for their growth, or that both of these conditions existed together. In all cases we have a period of incubation, a period of exacerbation, and a period of recrudescence.

In the milder exanthemata the organism must grow rapidly, and the return to health is usually rapid. The introduction of the poison into the system is by a different route, we admit, and this illustrates the "one-dose-of-poison" principle. In syphilis this order of things is materially changed, and we observe a longer period of incubation, a greater time again before exacerbation or the manifestation of systemic infection, and a still greater period to exterminate the poison by Nature, if, indeed, that is at all possible—which I doubt. Here we have the germ (?) introduced through a broken surface, developing at the very point of

introduction; this I believe to be capable of administering to the system, not *one dose* only, but many continuous doses of the poison.

The intensity of secondary syphilis has been held to be in direct proportion to the extent of the initial lesion. This I have corroborated, and I have further demonstrated, clearly to myself, that it is also in direct proportion to the time that the initial lesion has existed. This, it appears to me, demonstrates that it is in direct proportion to the quantity of germs introduced and the poisonous toxin generated by their growing in the system. I do not look upon the adenitis, as it appears during the continuance of the chancre, as due solely to the syphilitic poison, but to a double cause—the syphilitic germ plus an inflammatory condition due to the toxin; this is shown by the case here related, in which a subsidence occurred immediately on the removal of the sore.

I do not think that it is possible by any known means to abort the disease, once the germs in sufficient quantity have entered the general system; but I believe the course can be greatly modified by limiting the amount of the poison entering the economy. By early and free excision, the fewest possible number of germs are allowed to enter the system. In this way medical (or internal) treatment will have less to control.

Excision has been useful in other diseases in which fatal results follow the introduction of a poison, when neglected. The bite of the cobra has, by excision, been prevented from proving fatal. The excision must be practised early and freely to be successful. The effects of rabies have been prevented by free and early excision. Cauterization does not always produce the desired result. Here we are starting a new inflammation, and unless every germ is destroyed the cauterization is useless. We are producing a very suitable soil for these germs to propagate in. The same may be said of cauterizing the wound after excision, unless it be done with, say, pure carbolic acid, which does not prevent primary union.

We should excise freely and bring the edges together, and expect union by first intention. In these cases I believe that undoubtedly some poison must have entered the system, but not sufficient to be a dangerous dose. We have been shown by Biondi, Senn, Watson Cheyne, Hauser, and others, that there is a minimum dose of microbes necessary to produce systemic effects. Small doses produce no effect, and larger ones produce a greater and more rapid effect. Why not the same with syphilis?

The entire ablation of any localized diseased area is a sound surgical procedure. In these days of antiseptic and aseptic surgery it is the duty of every surgeon to render all diseased areas as nearly as pos-

sible aseptic. When excision is impossible, it is likely that free scraping may be practised and the same result attained. The free and early excision of the chancre changes an infected area into a healthy one, and removes a reservoir of growing germs that would otherwise have to pass through the entire economy. In a very short time after the chancre has cicatrized we observe the inguinal adenitis gradually subside until it attains the characteristic bullet-like hardness, and as the induration melts away this hardness also disappears, until we have left only a slight enlargement as compared with that in the first stage.

I wish it to be distinctly understood that I am not advocating the excision of chancres at all stages as an abortive measure, but as a means of lessening the systemic effects. I say again, if it is possible to see the lesion during the first few hours of its existence and to at once excise it, the disease may be aborted. Even with the ameliorating results, I do not allow my patients to be careless about internal treatment. I tell them that the treatment must be thorough, but I feel justified in assuring them that secondary results will be fewer in number and less severe in character as a consequence of the excision, and I encourage a hope that they may not appear.

I shall now report a case in which I think the disease was aborted.

CASE II.—J. McK. consulted me June 18, 1893. He noticed that morning a slight crack on the free border of the prepuce. Fifteen days previously he had had sexual intercourse with a woman of the town. He was under the influence of liquor at the time. On awakening in the morning, he noticed that she had a "skin-disease on her body." He did not give the matter any particular attention, other than careful inspection of the parts. I examined the woman within two hours, and found her suffering from a secondary skin-lesion and mucous patches on the vulva. I advised an immediate and free excision of the spot, which he willingly submitted to. The operation was done with antiseptic precautions and the edges of the wound brought together with fine silk. The wound healed by first intention. No hardness developed in the cicatrix, nor have any symptoms presented themselves up to April, 1894, when I last examined the man.

This case, it appears to me, furnishes as clear evidence of a disease aborted as any that has been published; yet the evidence is in some respects unsatisfactory. At the present time it is absolutely impossible to make a differential diagnosis by the microscope between a chancre and a chancroid. This case may go a long way in determining the course we should pursue in the treatment of the lesion, yet it offers no positive proof that it was syphilitic in character, although the presumptive evidence is very strong.

In six other cases the history is much the same. The wounds healed by first intention in five of them. In the sixth the wound cicatrized after some delay, as the sore had involved the border of the glans, and I did not thoroughly remove the whole of the infected area. The sores had existed for from four to fourteen days. The rash appeared very mildly in all of the cases in from forty to fifty-five days from the appearance of the chancre—four entirely escaped any other symptoms. Two had only very slight and easily-controlled mucous patches on the tonsils, but both of these patients had been inveterate smokers and had continued the habit. At least two years have elapsed since the last of this series of chancres was excised, and during the past eighteen months no secondary symptoms have developed in any of the cases.

In considering the subject of excision of the initial lesion it would be of great advantage if an answer could be made to the question, When is the system infected?

First. Is it immediately on inoculation?

Second. Is it during the incubation-period, *i. e.*, prior to the development of the lesion?

Third. Is it subsequent to the development of the lesion?

The first of these—Is it immediately on inoculation?—would, of necessity, open a purely theoretic argument for which at the present I have not the time. I do not believe that it is. All germs require time and a suitable medium for their successful development, the germ of syphilis requiring from seventeen to sixty days—a rather wide range. I know of no means whereby we can detect the slightest evidence of systemic infection immediately after the most suspicious intercourse. Nor on the closest scrutiny of many observed cases can the slightest abrasion be discovered.

In reference to the second—Is it during the incubation-period, prior to the development of the initial lesion?—again, I believe not. That period of time only has elapsed in which the germ has been able to reproduce itself in sufficient numbers to become manifest, and that at the very seat of inoculation. We have seen that a minimum number of germs are necessary to produce any result. I believe that that number only is attained when the lesion first appears. From this time the increase is fairly rapid. If it were possible to at once excise this spot I believe the system would not be infected, because sufficient of the germs have not been carried into the general system. It cannot surely be argued that the poison had entered the system, been carried throughout the whole economy and back to the very point of entrance to show itself there and there only. If the system was poisoned before the initial lesion developed, why do not lesions develop on any

sore or abraded spots that might have existed on the body during this time? If the system was infected prior to the appearance of the sore, why does it take on an average forty-two days longer for the roseola to show itself? Adenitis cannot be discovered before the appearance of the lesion, nor for some shorter or longer time after its appearance.

In reference to the third question—Is it subsequent to the development of the initial lesion?—I answer yes to this. Immediately the germ has developed sufficiently to produce the lesion, the irritation commences and the lymphatics at once take up the germ or its resulting toxin. The system is now infected, but with the very smallest quantity of poison. This is rapidly augmented and the poisoning becomes more severe. If now the sore is freely excised, possibly not enough poison has entered to produce a systemic effect, but this time must be counted by hours, not days.

The most important and most interesting question from a clinical standpoint now presents itself: Does the number of germs that enter the system in any way affect the subsequent course of the disease? If we can demonstrate this point, we have accomplished a great deal for the syphilitic sufferer. It does not sound irrational to say that a little poisonous material introduced into a suitable medium should take a longer period to contaminate the whole system than would a greater quantity. Nor does it seem to me to be irrational to say that a mild attack of any disease should be more easily controlled than a severe one. The syphilitic germ, like all others, propagates itself in proportion to existing numbers, *i. e.*, after the incubation-period has been passed there are thousands where tens existed before. Therefore we should take away any source of an increasing supply and allow the smallest number of germs to propagate themselves in the system. Nature tries to surround this infected area with leukocytes to devour the irritating and poison-producing germs; but they will not be devoured. She stands by at the glands to arrest them, but without success. Even mucous patches that occur in the mouth and on the tonsils are treated locally as much to prevent systemic reinfection as to remove the unpleasantness caused by their presence. I have endeavored to show that fewer secondary manifestations appear in those from whom the chancre has been excised than in the ordinary case in which excision has not been performed, and if this is the case, then excision is not only justifiable, but advantageous.

I have tried to base my remarks as much as possible on clinical cases, and have not attempted to quote authorities, with whom quite possibly you are more familiar than I am. I advocate the free excision of the sore; that the wound be not cauterized,

except possibly with pure carbolic acid, and that the edges be united; that the operation be done with the strictest antiseptic precautions, including the after-dressing.

I would draw the following conclusions:

First, that the early excision of chancres—that is, within a few hours after their appearance—will abort the disease.

Second, that the excision of any unhealed chancre will moderate the subsequent secondary manifestations.

Third, that excision constitutes the cleanest, least painful, and most scientific method of treating the lesion.

TORONTO: 61 QUEEN STREET, EAST.

A UNIQUE FORM OF MOTOR PARALYSIS DUE TO COLD.

BY EZRA CLARK RICH, M.D.,
OF OGDEN CITY, UTAH.

I WISH to call attention to a family peculiarity existing in a number of my own relatives. At the present time there are living in the southern part of the State of Idaho fourteen individuals whose nervous system is peculiarly susceptible to the influence of cold. I recognize the fact that cold is a very important factor in the production of certain forms of neuritis which may result in paralysis; but paralysis without a neuritis or inflammation of any of the tissues of the body, is, I believe, a rare condition. I suppose the nervous system of almost all individuals is more or less depressed by the effects of cold, and in some it may result in slight paralysis, as of one or more fingers. Paralysis of entire groups of muscles, however, as the whole of both upper or lower extremities, without the symptoms and results of inflammation, is perhaps only found in those possessing the peculiarity which I wish here to describe.

In fourteen persons now living with whom I am well acquainted, as it was also in three who are dead, the condition is the same under similar circumstances. When brought in contact with a cold, damp atmosphere, the muscles most exposed soon become fixed and immovable, the paralysis being due to tonic spasm. The parts exposed may be fixed either in extension or in flexion. The muscles of the face are most frequently affected. The accompanying photograph, Fig. 1, shows fixation of the facial muscles; Fig. 2 shows the same individual as she is normally. The attitude may differ from this; one eye may be open and the other closed. One side of the face may be drawn up and the other be straight, or the face may be paralyzed in the act of laughing, crying, whistling, etc. I have seen one of these individuals affected while laughing and the facial muscles remain in the

attitude of laughter for twenty minutes or more, and then be only relieved by getting into a warm room. The arms are quite often affected, and come next in frequency to the face. The muscles of the fingers and forearm are generally, if not always, affected first, as a result most likely of their being most exposed. Both arms may be paralyzed at the same time, or one arm, one hand, or one finger may be

FIG. 1.



FIG. 2.



affected, and the rest of the body normal. The same is true of the lower extremities; the muscles of one or both thighs may be attacked, and the legs be normal, or one or both legs may be affected, and the thighs and rest of the body be normal. All the groups of muscles mentioned may be affected at the same time, making it impossible to move even a finger or a toe, much less an arm or a leg.

It may not be out of place here to relate a circumstance occurring in the history of one of these individuals. After dancing for several hours and

becoming tired, her underclothing becoming saturated with perspiration, she retired for the night with a sister who was not affected with this peculiarity. The room was damp and there was light covering on the bed. The girl did not remove her damp underclothing, and was soon completely paralyzed, with the exception of her tongue, which on account of the warmth of the mouth escaped. While in this condition she relates that she was unable to move any part of her body except her tongue, by which she made her condition known. Respiration was not materially interfered with. The intellect was clear, and conversation could be carried on as well as other times, except that the lips were paralyzed. Sensation was not impaired, and she suffered no pain. Recovery was brought about by the application of heat, and in five or six hours she had completely recovered, without any ill effects from the previous condition. Severe forms of this paralysis, as under the circumstances just related, are not at all common; but fixation of certain groups of the muscles of the face, arms, or legs, is very frequently seen during cold, damp weather. I have repeatedly seen different individuals of this family with their upper or lower extremities in a fixed condition from cold, and no more able to move the affected part than a chronic invalid who has been paralyzed for years. The tongue is very easily affected by taking snow or ice in the mouth. When any amount of this is swallowed, the muscles of deglutition are affected and swallowing becomes impossible.

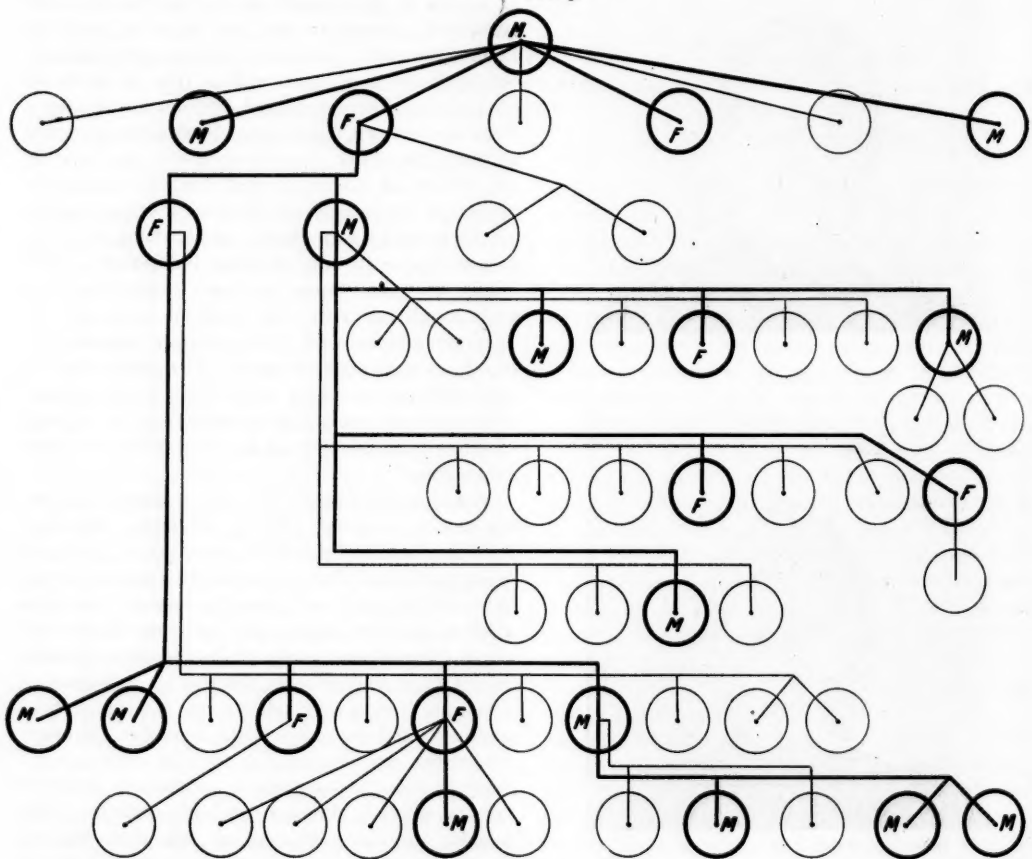
The affection seems to be only a motor disorder, the sensory nerves not being involved. Dr. E. I. Rich has carefully tested the sensation of the paralyzed parts, and finds it not in any way impaired. When the tongue is affected the sense of taste is as acute as at other times. At the onset of the paralysis there are no symptoms in the parts affected, except what the subjects describe as a "drawing sensation," especially felt in the anterior part of the thighs and the calves of the legs when the lower extremities are affected. A peculiar symptom occurring with the paralysis is an intense desire to urinate, which is almost a constant symptom. The bladder can be evacuated at this time without trouble, and the desire is not repeated. Paralysis of the sphincter muscles of the bladder and rectum, with uncontrollable evacuation of the contents of the bowel and bladder, has never happened as far as I have been able to ascertain, and I have made careful and especial inquiry regarding this matter.

Recovery from this condition is always complete, and is brought about by the application of heat, either as warm air or as the warm bath. There is no pain either accompanying the fixation of the muscles or during the recovery. In the slight forms

of the affection, as seen in Fig. 1, recovery would take place in about half an hour. When the tongue is fixed recovery is complete in a few moments; but in the severe forms five or six hours, under favorable circumstances, will elapse before the parts have recovered their strength. The return of strength is gradual; the affected part is slightly swollen or edematous and very weak and limp, but not tender to the touch. When the tongue is affected it is very much increased in size, almost entirely filling the mouth. The marked edema of this organ is probably due to its great vascularity.

the vitality of the system, as present or previous disease, exhaustion from overwork, or want of food, etc., predisposes the individual to the affection. Muscles in active motion are not affected. In walking, the arms and face may become paralyzed, while the legs would not be affected as long as they were kept in motion. If however, they were tired and the individual should sit down for five or ten minutes his legs would almost surely stiffen. I do not wish to convey the idea that these people are paralyzed every time they come in contact with cold air; on the contrary, they are sometimes able

FIG. 3.



After recovery from the severe forms of paralysis the system seems exhausted for a few hours. The feeling is described by the patients as one of "gone-ness"; nausea is very common, but vomiting rarely occurs. There are no sequelæ, notwithstanding the number of times these people are affected every year; there have never been any bad after-effects. The condition seems evidently one, as already stated, of motor paralysis without inflammation or its consequent results. Any condition that would lower

to stand a great deal of cold weather without being affected by it, and on the other hand they are frequently affected in the summer during damp, cold, weather. Their susceptibility seems to depend entirely upon the amount of resistance which the body has to offer.

One of the most interesting features of this condition is its distinct heredity. I am able to trace the condition into the fifth generation (See Fig. 3.) I start with T. B. C., born in Milford, Connecticut,

in the year 1773. He was one of quite a large family, two or three of whom besides himself had this peculiarity. He had a family of seven children, of whom two sons and two daughters inherited this affection. Of three of the four I am not able to give any history at the present time. The other child, one of the daughters, married and had four children, three daughters and one son. The son and one daughter inherited the peculiarity. The writer's mother was one of the daughters who did not inherit the affection. The affected male of this family was married three times. By the first wife he had eight children. Of these two sons and one daughter were affected, and one of these three, when a boy of two and a half years old, died of diphtheria. The other son is now married and has two children not affected. By the second wife he had seven children. Two daughters have the peculiarity; one of these is married and has one child not affected. By the third wife he had four children, one son having the affection. This makes nineteen children in all, six of whom inherited the peculiar form of paralysis. I now return to the daughter in the family of four. She married and had eleven children, and of them three sons and two daughters have the affection. One of the sons is married and has a family of five children, three sons being affected. The daughters are married; one has a family of five children, one son inheriting the peculiarity. The other daughter has one child not affected. This makes a total of fourteen now living (nine males and five females) and eight who are dead who have inherited this peculiar form of paralysis. I am told that there are some members of this same family living in Connecticut who have this affection, but I have not been able to get their address.

I believe the foregoing history of this curious form of motor paralysis establishes its heredity beyond a doubt. The peculiarity never skips one generation and appears in the next. The family is aware of that fact, and knows that when a child does not inherit the affection it is ended as far as his offspring are concerned. The history of the family seems to prove this statement. I have never seen anyone outside of this family having this peculiarity, and in searching for literature on the subject have not been able to find anything similar. The only affection which at all resembles the condition described is Thomsen's disease. Like the latter, it is distinctly hereditary; but it differs from this in that it is always brought on distinctly by exposure to cold, and lastly, while in Thomsen's disease voluntary movements bring on the spasm, in the affection here described voluntary movement absolutely prevents the attack.

I have known and associated with these people all my life. I have seen different ones affected

numbers of times. Their condition is well known to the general public living in the section of country with them. They speak of their condition as "numbness" or "getting numb." Living as they do, in a climate where the ground is covered with snow for three or four months in the year, and where a temperature of 20° or 30° below zero is not very uncommon in winter, it can be readily understood that they know the necessity of taking care of themselves, and that exposure under certain circumstances in which other people would not be in danger would be simply suicidal on their part. There is no other peculiarity in this family that I am aware of. They are not of a highly nervous temperament; they are nearly all engaged in farming and stock-raising, this causing them to lead out-door lives. There has never been a case of hysteria, mania, locomotor ataxia, infantile paralysis, multiple neuritis, or any of the allied diseases in the family. One of the fourteen, a very strong, healthy woman now, had chorea when a child. The disease yielded readily to treatment, and has not appeared since. It seems hardly necessary to say that they are not suffering with any form of specific disease or that they are not alcoholics.

As to the nature of this affection, I believe there is little to be said. At present there has been no histologic investigation made of their nervous or muscular tissues. The absence of pain and the fact that sensation is not at any time impaired seems to eliminate the sensory side of the nervous system from being implicated. Still it may be possible that the peculiarity exists in the muscular fibers, and that the spasm is the effect of reflex action. It is more probable, however, that the affection is one of the end-plates of the peripheral motor nerves, and that the cold has its peculiar influence on this part of the nervous system. The edema of the part seems to indicate that the vasomotor nerves supplying the walls of the vessels of the part affected are implicated, allowing the serum of the blood to pass out into the surrounding tissues.

CLINICAL LECTURES.

POSTERIOR SCLEROSIS AND DILATATION OF THE STOMACH IN THE SAME PATIENT.

BY J. M. ANDERS, M.D., PH.D.,
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PHILADELPHIA.

[Reported by A. M. DAVIS, M. D.]

A. J., a brushmaker, fifty-six years old, gives a negative family and personal history. Five years ago, having taken two meals at one sitting and thus gorged himself with food, the man was seized with sharp, lancinating pains in the epigastrium, extending to the back below the level of the angle of the scapula, and accompanied by nausea and vomiting; the vomitus being thin and watery, mixed

with mucus, and containing particles of undigested food; it was light in color when first ejected, but became dark on standing. Medicinal treatment at that time failed to relieve the symptoms to any marked degree. The attacks were followed by intermissions, recurring usually at intervals varying from a few days to several weeks, and persisted for the same length of time. Later paroxysms were unattended with pain, but were accompanied by a sensation of constriction or pressure across the epigastrium, with nausea and vomiting. Just prior to admission sharp, shooting pains referred to the lower extremities were complained of at the time of the attacks, which still persist. Loss of flesh and strength has been steadily progressive, the bodily weight being fifty pounds less than before the onset of the disease.

On admission to the hospital (two weeks ago) the bowels were constipated, the skin harsh and dry, the urine diminished in quantity and high-colored. Vomiting has occurred daily, the amount being large (sometimes exceeding a basinful), and consisting of a watery, mucoid liquid, with a very sour, foul odor. Repeated physical examinations showed unmistakable signs of dilatation of the stomach. On inspection the fundus was invisible, the peristaltic wave being absent. Palpation elicited rumbling noises from the presence of flatus, but no splashing was detected. Percussion yielded a hollow, low-toned tympany, which extended fully two inches below the umbilicus, reaching below a transverse line drawn from one anterior superior iliac spine to the other. On distending the stomach with carbon-dioxid gas (by administering separately one-half dram each of sodium bicarbonate and tartaric acid in solution) sizzling or hissing sounds were heard below the umbilicus (as from a siphon charged with soda-water); and on having the patient swallow a small quantity of water, rushing sounds were plainly audible. To demonstrate more clearly the presence of dilatation, an esophageal bougie was passed for a distance of 63 cm., the end being palpable through the abdominal wall below the navel. In a viscus of normal capacity the length of the inserted tube should never exceed 60 cm. Chemic examination showed a total absence of HCl in the filtered gastric contents, with an excess of organic acids (lactic, butyric) and fermentation-products.

The pupils are small and unequal, myosis being the more marked on the left side. They are unaffected by light, but respond readily in accommodation, thus constituting the Argyll-Robertson sign of posterior sclerosis. There is no diminution, however, in the field of vision, and ophthalmoscopic examination fails to reveal evidences of optic atrophy. Slight temporary ptosis is apparent on the left side, but no strabismus. Examination of the lower limbs shows almost total absence of the patellar reflexes, and on having the patient stand erect, with his heels in contact and his eyes closed, an uncertain swaying or rocking motion is observed, showing some loss of coordination. His gait is characteristic, the legs being held widely separated, the left foot being thrown out forcibly, and the heels striking the ground first in their descent. Some loss of muscular sense is apparent, *i. e.*, the patient does not know how much power is required to walk.

The etiologic factors in the case are of the utmost importance. Ferrier believes *tabes dorsalis* to be due in

every instance to syphilis, either recent or remote. Male sex, middle life, chronic alcoholism, prolonged physical labor, and repeated exposure to cold and wet are among the predisposing causes. In this patient, after considerable difficulty, we have recently obtained a clear history of specific infection of twenty years' standing.

In other well-attested cases, typical symptoms of posterior sclerosis have followed ergotism and poisoning by carbon disulphid, lead, arsenic, and other mineral poisons. It may also follow spinal injuries, or develop during the course of wasting diseases. The gastric dilatation present had for its exciting cause over-distention produced by gormandizing, though there had probably existed a predisposition in this instance.

During the course and progress of *tabes dorsalis*, three stages or groups of symptoms present themselves—the *preataxic*, the *ataxic*, and the *paralytic* stage, the symptoms present in this case characterizing the first or pre-ataxic stage. Pain, which is the most marked symptom, often persists throughout the course of the disease, or it may end with the onset of ataxic symptoms; it is of a shooting or electric type, often being referred to the lower dorsal region. Later the lower limbs become affected. In this instance the patient suffered severely with attacks of pain in the lower extremities, which he attributed to rheumatism or neuralgia. Intense pain may also be complained of in the rectum, bladder, and stomach, together with band-like sensations (as of a tight belt) about the waist.

Sensation is disturbed early; there may occur feelings of formication in the hands and feet, accompanied by profuse sweating. Areas of paresthesia and anesthesia often develop, appearing first upon the thighs and perineum, and later involving the remainder of the lower, and finally also the upper extremities. Polyesthesia when present is highly characteristic, one prick of a pin giving rise to several painful impressions. Later in the course of the disease, anesthesia of the muscles, tendons and fascia is present. The deep reflexes are usually early affected, although if the lesion be limited to the dorsal cord, and the lumbar enlargement be not involved, well-marked cases may exist without any diminution of the knee-jerks; on the other hand, the patellar reflex is often absent in health and in many cases of neurasthenia.

Vasomotor and trophic disturbances not uncommonly occur during the pre-ataxic stage; sweatings (localized and general), edema of the extremities accompanied by herpetic eruptions, and in some instances alopecia and shedding of the nails, occur. A peculiar enlargement of the joints, unaccompanied by inflammatory symptoms, is highly characteristic. To this condition has been given the name of "Charcot's joint." When occurring in the knee, serum may be present in the synovial sac and float up the patella, but the fluid finally becomes absorbed. Partial ankyloses may result from absorption of the ends of long bones, lateral motion and crackling sounds being elicited on manipulation; the knee-joints are thus commonly affected.

If the sclerotic process affects the anterior cornua, paralyzes result; peripheral neuritis with muscular atrophy and degeneration occurring only when other structures are involved. In this case there is much emaciation, but entire absence of joint-involvement or of symptoms pointing to motor palsy.

Ocular phenomena are equally important, the Argyll-Robertson pupil, with some ptosis, being present in this case. In some instances rapid diminution in the field of vision appears early, with color-blindness, followed by the gradual development of optic atrophy. Sometimes hearing is interfered with, and the senses of taste and smell may be perverted or blunted. Aberrant sensations are not uncommon; thus, in walking, the patient often feels as though the soles of his feet were treading on feathers, cotton-wool, or fur, a symptom that is, however, not present in this case.

The foregoing group of symptoms may almost disappear at the end of the first stage, or may persist throughout the course of the disease. The ataxic period is characterized by the peculiar gait which, when well developed, is pathognomonic of tabes. As the case advances, the peculiarity becomes more and more pronounced. To assist coördination the eyes are kept on the feet when walking; later, a cane has to be employed, and as the paralytic stage is reached the patient uses crutches or is obliged to resort to a wheeling-chair. During the second stage visceral crises are more common, and may affect the larynx, stomach, or kidneys. When occurring in the larynx, severe attacks of cough, with dyspnea and dysphagia develop. Pain and laryngeal spasm may be intense, and attacks of true angina are not uncommon. The gastric crises are characterized by intense pain after partaking of food, with violent vomiting and purging; these are present in this case. The vomited matter is small in amount, intensely acid, and has been termed by the patient "pepper vomit," in contradistinction to that due to the dilatation, which occurs in large amounts at one sitting. It is a true ataxic vomit of nervous origin. Renal crises much resemble in symptomatology attacks of renal colic. In the former, however, blood and pus never appear in the urine, and this fact, together with the occurrence of other visceral crises, the Argyll-Robertson pupil and abolition of the reflexes, tends to establish the diagnosis. When, finally, the paralytic stage develops, the patient becomes bed-ridden, and may remain many years in this deplorable state, unless carried off by some intercurrent disease.

In the beginning or *pre-ataxic* period of this disease a positive diagnosis is rarely justifiable unless ocular symptoms are present, and, when existing, the previous history of the case should always be taken into consideration. The presence or absence of patellar reflexes is of little consideration; for, as already stated, they may be abolished in a case of neurasthenia, or they may even be absent during health. If, however, lightning-like pains in the extremities are present, with an absence of the reflexes, the presence of the dread disease may be suspected.

Certain conditions may simulate locomotor ataxia. Thus, in *diabetes mellitus* there may be pain and tenderness over certain nerve-trunks with paralysis of the extensors (especially of the legs) and absence of the patellar reflexes. In this disease, however, we have a different history, with the existence of glycosuria; ocular symptoms are absent and the gait is not ataxic in character. *Alcoholic ataxia* often resembles that of posterior sclerosis, but there is a distinct history of alcoholism, together with the presence of the coarse tremor increased

on voluntary effort, with an absence of eye-symptoms, and usually rapid improvement following treatment. *Peripheral neuritis* is associated with a peculiar 'steppage' gait; the extensors of the legs being paralyzed, the feet are lifted high, to clear the toes of the floor, the plantar surfaces being brought down flat. The gait of *cerebellar ataxia* is sometimes difficult to discriminate from that of posterior sclerosis; but in the first-named condition the knee-jerks are retained; there is usually great pain at the base of the brain, with persistent vertigo and vomiting, unaccompanied by nausea. Optic atrophy or "choked disc" also develops often early in the course of this affection. In *diphtheric paralysis* the gait is sometimes much like that of true ataxia; the different history and the paresis of the throat-muscles would aid, however, in making a differential diagnosis.

The prognosis of locomotor ataxia is decidedly unfavorable as to recovery. Many cases, having reached the paralytic stage, persist for twenty-five years. Often uncertain symptoms of the pre-ataxic stage last ten years before the development of ataxia. Only when the history of syphilis is recent can the prognosis be said to be at all favorable, and even then apparent cure is often followed by a relapse, with a fatal termination. From some unknown cause, cold weather seems to be unfavorable to this disease, whereas temporary improvement often occurs during the summer months.

In the patient before us the two conditions present demand separate and specific methods of treatment. On admission, lavage was promptly resorted to, the stomach being irrigated twice daily with a 3 per cent. solution of boric acid. In performing lavage in the treatment of dilatation of the stomach it is often necessary to employ a larger tube than that of ordinary caliber, because of the lodgment in and obstruction of a smaller tube by undigested particles. At the present time irrigation has been discontinued, and the patient is taking four times daily a pill composed of silver nitrate and extract of hyoscyamus, each gr. $\frac{1}{4}$. The use of the former drug may be safely continued until upward of two hundred grains have been taken. Anti-syphilitic treatment should also be begun early and persisted in. Mercurial inunctions may be employed continuously for a period of two months, together with small doses of potassium iodid (from 5 to 10 grains) three times daily for the same length of time. These may then be discontinued for one month, and be again repeated for the same period. The use of certain drugs has been shown to have an injurious effect upon posterior sclerosis; among these may be mentioned belladonna and ergot, ergotism frequently giving rise to symptoms resembling those of locomotor ataxia.

For the severe dorsal pain present, cupping along the spine often gives great relief. Many authorities claim great benefit in the treatment of this disease by the use of electricity, the faradic brush, or the constant current being applied along the spinal column.

The suspension-treatment, formerly much advocated, often does harm, and has fallen into disuse. It should never be employed when marked prostration or organic heart-disease exists.

In conclusion, it is interesting to consider whether or not the two conditions coexisting in this patient—*i. e.*, gastric dilatation and posterior sclerosis—bear any rela-

tion to one another, and, if so, how that relation is brought about. Though the matter cannot be positively determined, it is probable that locomotor ataxia was here the primary disease, and this may have served as a predisposing factor to gastric dilatation. So far as my knowledge extends this has not been mentioned by any authority as a cause of dilatation of the stomach; neither have I met with any records of cases in which the two diseases under consideration appeared together. Furthermore, it is not improbable that the tendency to gastric dilatation in this case was dependent to a greater or lesser degree upon degenerative processes in the peripheral nerve-supply of the stomach. It is well known that the sclerotic changes may extend from the posterior to the anterior cornua, an explanation being thus afforded for the parietic symptoms present in these cases.

GONORRHEA IN THE FEMALE; CYST OF THE BROAD LIGAMENT.

Delivered at the Buffalo General Hospital.

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GONORRHEA IN THE FEMALE.

THIS patient was brought into the hospital a few days ago. Upon examination it was found that she had gonorrhea. On attempting to pull down the cervix with a tenaculum the tissues tore through like wet paper. I at once thought of pregnancy, and on inquiry learned that the patient had not menstruated for some months. The color of the cervix was also suspicious. There were a number of venereal warts which I tried to remove, but could not, as the attempt caused considerable pain. I have, therefore, had her anesthetized in order to remove them, and also to make a more thorough examination in your presence. The abdomen presents no striae; the hymen is nearly intact; the cervix is not lacerated; so it is evident that the patient has not borne children. The vagina has not the pink color of the healthy non-pregnant state, but is more or less blue, and you will note the characteristic purple of the anterior wall. The cervix is soft and velvety, and by bimanual palpation I can find something between my hands, and now I get *ballotement* as typically as I have ever felt it.

It is an interesting point, and one of importance so far as prognosis and treatment are concerned, to know whether the pregnancy antedated the gonorrhea, or *vice versa*. It is very doubtful if gonorrheal pus can invade the pregnant uterus by gaining access between the membranes; and, if gonorrhea reaches the endometrium and tubal epithelium, pregnancy is not likely to occur. Venereal warts are more prone to develop and to grow rapidly during pregnancy. Gonorrhea does not affect the vagina materially; in fact, some claim that gonorrheal colpitis has no existence. You can see in this case that the warts grow from the vulva, and scarcely at all from the vagina. By nipping off the warts and cauterizing their bases, this manifestation of the disease can easily be removed, and in two or three days we shall make applications of silver nitrate to the vagina, taking advantage of the occlusion of the uterine cavity by the fetal membranes to eradicate the disease.

I have seen several cases of puerperal fever that were undoubtedly due to gonorrheal infection extending from the cervix. Tait says that for every thousand fatal cases of syphilis in women there are tens of thousands of fatal cases of gonorrhea. It would certainly take hundreds of ordinary cases of syphilis to cause the same amount of agony that is produced in one case of gonorrheal pyosalpinx. Syphilis in the male is a curable disease, and in women it is rather likely to run a mild course. It is not once a year that I meet with a case of syphilis in my private practice. Gonorrhea, on the other hand, enters into about half the cases of pelvic disease that I see. Just at present I have under my care a young, healthy-looking girl, married a month ago to a man of the world. She was suddenly attacked with pelvic inflammation without any good reason, although there is a story about her catching cold while menstruating, the window being open at night. I have found a salpingitis of the right side, and a green, peculiar-appearing discharge from the cervix. I have no doubt but that the man has had a gonorrhea, and that he has infected the girl.

Our attention was first forcibly called to the matter of gonorrhea in women by Dr. Noeggerath, of New York, who in 1876 read a paper on the relation between gonorrhea and pelvic disease. His statements were laughed at at the time, and, indeed, he has modified them since; but his views were largely correct. He went so far as to claim that no man ever recovered from gonorrhea, and, therefore, that no man who had once had gonorrhea could ever have children. After Noeggerath's paper was published several physicians corroborated his statements, to a greater or less degree, but it was not until Tait showed by the results of his abdominal sections that pyosalpinx, pelvic peritonitis, and a multitude of complications are due to gonorrhea, that the truth which lay in Noeggerath's statements was accepted. It certainly is too sweeping a statement to say that gonorrhea is never recovered from. I remember being on a hunting excursion once with a young man whose woes were testified to by the numerous rags which fluttered on the bushes at each camping place; yet he has since married and had children, and his wife is a perfectly healthy woman.

CYST OF THE BROAD LIGAMENT.

This specimen is a cyst of the broad ligament, which I removed some days ago. It is known to be a cyst of the broad ligament for several reasons: 1. The ovary is found intact and separate from the cyst, as you see. The fimbriated extremity of the Fallopian tube is found in the middle of the sac. 2. The fluid contained in the cyst is clear, and not thick and cloudy like the typical contents of an ovarian cyst. 3. Finally, the proof positive that we have a cyst of the broad ligament is that the whole tumor is covered with peritoneum. It is sometimes possible to enucleate such a tumor without leaving any pedicle at all, but usually there are numerous bands of adhesion, the remains of inflammatory processes, which make the removal of the cyst a matter of difficulty.

This other specimen, curiously enough, seems also to be of the nature of a broad-ligament cyst. The whole pelvis was filled with a mass which was more or less

cystic, and by vaginal examination a number of little soft cysts could be felt behind and at the sides of the uterus. On opening the abdomen and getting my hand into the pelvis, I found an immense mass of adhesions, the tumor being bound down behind the broad ligament as usual. I could break up the adhesions, however, by reaching down behind the mass and raising the hand so as to imitate the action of a paper-cutter. This is the usual maneuver. This appendage I take to be the ovary, and as it can be separated from the wall of the cyst, the cyst must be intra-ligamentary. The sacs are filled with clear fluid. The Fallopian tube of the opposite side was also distended with fluid, and was removed with some difficulty, as it was so low in the pelvis. The ovary was so firmly adherent that I could not get it out. We had to deal, then, with a hydrosalpinx of one side, and a cystic degeneration of a number of follicles of the body of Rosenmüller on the other side.

CLINICAL MEMORANDUM.

SUPPURATIVE METRITIS; ULCERATIVE ENDO-METRITIS AND METRITIS; DOUBLE PYOSALPINX AND AN OVARIAN ABSCESS; EMBOLIC PNEUMONIA FOLLOWING LABOR; HYSTERECTOMY; RECOVERY.

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THE patient whose case is herewith reported was admitted to the Philadelphia Hospital a month after her delivery. She gave a history of having been ill and feverish since the childbirth. She had been taken from a squalid hovel, where she had received no care. At first glance it seemed apparent that she had not long to live. The temperature was high, the pulse rapid and feeble, the respirations hurried and shallow, the abdomen tympanitic and exquisitely sensitive, and there was profound prostration. On vaginal examination the uterus was found large, boggy, firmly fixed, with sensitive masses behind and on each side. Over a considerable area of

FIG. 1.



Anterior view: Perforation at fundus; abscess opened, to the left; abscess unopened, to the right; double pyosalpinx.

the right lung, especially toward the apex, there was diminished resonance, together with numerous fine râles. The abdomen was opened as soon as practicable. At the fundus of the womb a perforation was made into the

cavity by pressing lightly upon it with the finger-tip. The ulcerative process had destroyed the muscular wall to the peritoneal covering. On both sides of the uterus, about an inch below the insertion of the tubes, were large abscesses in the uterine wall. Both tubes were distended with pus, and there was a large ovarian abscess

FIG. 2.



Posterior view: Large ovarian abscess.

on one side. The broad ligaments were much thickened, being quite an inch thick at their base. On this account the removal of the womb and the control of the hemorrhage was extremely difficult. The patient reacted well after the operation, and made a good recovery, although her convalescence was somewhat delayed by an abdominal fistula that finally, however, closed spontaneously.

It would be scarcely possible to find a more unfavorable case of puerperal sepsis for radical operative treatment. The successful issue is encouraging as an evidence that one need not necessarily despair, even in advanced cases and in involvement of important structures, necessitating the removal of all the genitalia above the vagina or above the cervix. A cure, however, by surgical treatment, especially if hysterectomy is required, is and must remain exceptional. The recent literature upon this subject, unless carefully studied in the light of personal experience, is misleading. Hysterectomy has been done successfully for puerperal sepsis when it was not at all necessary. I saw last winter two perfectly healthy wombs removed for fever after childbirth. The unfortunate women, unnecessarily mutilated, would have probably recovered much more rapidly, and certainly much more safely, under intra-uterine disinfection and stimulus and support. Several of the reported cases are also open to this criticism. On the other hand, a number of fatal hysterectomies for puerperal sepsis have not been reported. Eliminating the unnecessary operations, and, were it possible, collecting all those that have been unsuccessful, it would appear that hysterectomy for puerperal sepsis has a frightfully high mortality. But even if but one case out of a hundred is saved, the operation deserves credit, for without it, in a case really requiring the removal of an infected womb, the patient must inevitably die.

An important problem in this connection is the decision whether in a given case operative treatment promises something for the patient or is surely foredoomed to failure; and, again, whether operative treatment is really required. These questions cannot always be answered till the abdominal cavity is explored after incision of its walls. But the following rule governs my own action at present:

If there is no evidence of extension of inflammation beyond the womb itself, do not operate.

This at once excludes a large number of cases: diphtheric endometritis; suppurative metritis, in which the abscess is nearer the uterine than the peritoneal cavity; catarrhal and suppurative endometritis, in which the tubes are not involved, to the extent, at least, of closure and collection in them of pus; phlebitis; sapremia. In diphtheric endometritis the removal of the womb has in my experience had no influence on the course of the disease. I have, indeed, not yet seen such a case recover under any treatment. If the abscess in suppurative metritis is near the uterine cavity, it will probably discharge into the birth-canal, as I have seen it do. If, on the other hand, it extends toward the peritoneal cavity, exudate is poured out, the womb is fixed, and an indication is afforded for operation. The other varieties of sepsis enumerated are obviously not amenable to treatment by abdominal section. On the other hand, the septic conditions that have demanded operative treatment after labor are, in my experience, purulent peritonitis, localized or diffused; suppurative metritis; ulcerative metritis, threatening and actually causing perforation; pyosalpinx and ovarian abscess; suppuration of the infiltrated connective tissue in the floor of the pelvis and at the base of the broad ligaments. This last condition calls not for abdominal section in the ordinary sense, but for an extra-peritoneal incision above Poupart's ligament or through the vaginal vault.

THERAPEUTIC NOTE.

THE HYPODERMATIC INJECTION OF MAGNESIUM SULPHATE AS A PURGATIVE.

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In September, 1893, a paper on the hypodermatic injection of magnesium sulphate as a purgative, by Geo. H. Rohé, M.D., Superintendent, and J. Percy Wade, M.D., first assistant physician of the Maryland Hospital for the Insane, was read before the Pan-American Medical Congress, Washington, D.C.

The number of cases selected for observation was forty-six. The number of injections made was one hundred. In 67 per cent. the injection was successful; in 33 per cent. it failed. The dose varied from 1.86 grains to 4.5 grains. It was found that small doses acted as efficiently as slightly larger doses. In only one case was the larger dose (4.5 gr.) employed.

The shortest time for the injection to produce an evacuation was three hours, the longest fourteen.

In order to further test this remarkable property of magnesium sulphate twenty-five cases were selected, and fifty injections of from one to six grains were made. The average dose was 2.78 grains.

Whether a bowel-movement, occurring fourteen hours after the hypodermatic administration of magnesium sulphate in small doses can be considered as due to it, is, to say the least, an open question.

It will be seen that, even admitting the injection to be effective at the end of seventeen hours, in our cases the percentage of success is only eighteen, and that of failure, eighty-two.

CASE IV was usually constipated ten days, and in the habit of using enemata. The injection of one and one-half grains was followed by an easy movement in seven hours. Upon the following day, without any injection, two movements occurred.

CASE II.—On days upon which the injections were omitted, bowel-movements occurred, but it seemed that under treatment the stools were slightly softer in character.

CASES I, IX, XIV.—Subsequent observations showed the character and frequency of the stools to be the same without the exhibition of magnesium sulphate hypodermatically. In Case XIV increasing doses, from grains two to grains six, were used without any appreciable effect.

The question naturally arises: Are these bowel-movements solely due to the administration of magnesium sulphate hypodermatically, or would they have occurred without it? With the exception of two Cases II and IV, no change in the character or frequency of the stools was noticed. But considering all these cases as successful, the percentage of successes does not exceed eighteen.

In the majority of cases the results were negative.

In order to compare with the foregoing the action of magnesium sulphate given in the usual doses by the mouth, eleven cases were selected, and in these, with one exception, the results were negative with hypodermatic administration.

The results were as follows: after fifty injections there occurred in seventeen hours nine bowel-movements: one in the first hour; one in the second hour; one in the third hour; one in the fifth hour; one in the sixth hour; three in the seventh hour; and one in the ninth hour.

Taking a limit of fourteen hours as before, out of eleven cases, we have eight in which the results were successful, and two which responded in twenty hours, the character of the stools being appreciably altered. However, considering the latter as negative, our ratio is: success 72.7 per cent.; failure, 27.3 per cent.

Now comparing these results, with those of the hypodermatic administration, we have:

Hypodermatically: Success, 18 per cent.; failure, 82 per cent.

By the mouth: Success, 72.7 per cent.; failure, 27.3 per cent.

Although we do not consider the number of hypodermatic injections as conclusive, still, to us, the foregoing results cast considerable doubt upon the purgative property of magnesium sulphate exhibited hypodermatically in small doses.

In conclusion, we may say that the injections were made with solutions from tablets kindly furnished by Sharpe & Dohme, and that no local disturbance was produced, except in Case XXV, in which there occurred a small area of tenderness to pressure, which disappeared of its own accord in a few days.

Dr. C. B. Burr has resigned the position of superintendent of the Eastern Asylum, at Pontiac, Mich., to accept the superintendency of Oak Grove, a private asylum for the insane, at Flint, Mich. Dr. Burr is succeeded at the Eastern Asylum by Dr. E. A. Christian, formerly assistant superintendent.

MEDICAL PROGRESS.

Gastrostomy by the Method of Witzel for Carcinoma of the Esophagus.—TAIT (*Pacific Medical Journal*, vol. xxxvii, No. 6, p. 328) has reported the case of a man seventy-two years old, who for seven months had complained of dysphagia and dull pain in the inter-scapular and precordial regions. At first only solid food caused discomfort, but in the course of four months the swallowing of liquids also became difficult. In attempting to swallow, the patient felt a sense of obstruction at two points, the first, probably spasmodic, corresponding to the origin of the esophagus, and the second being referred to a point about two inches above the lower extremity of the ensiform cartilage. Regurgitation of food occurred frequently, and the patient had lost sixty pounds in weight. An esophageal bougie encountered resistance at a point twelve and one-half inches from the teeth, but with some difficulty passed the obstruction, which apparently measured an inch and a half in length. For three weeks, at the patient's request, bougies were passed daily, causing much pain, but effecting no good. Finally the patient assented to an operation, and gastrostomy was undertaken. An incision, two and one-half inches long, was made, beginning a quarter of an inch to the left of the median line, parallel to and an inch below the costal arch. The presenting anterior wall of the stomach was incised and a No. 26 catheter introduced into the viscus toward the cardiac extremity. The catheter was then enfolded for a distance of two inches by two rows of sixteen Lembert sutures. Four stitches were introduced into the walls of the stomach, thrust through the abdominal wall and made secure after inserting the silkworm-gut sutures for closing the abdominal incision. A sterilized-gauze dressing was applied and a clamp was placed over the outer opening of the tube to prevent the escape of the contents of the stomach. Rectal alimentation was practised for the first twelve hours. Sixteen hours after the operation a nutritive injection of four ounces was made through the tube into the stomach. On the second day the injections were given every four hours, and after the third day the amount was doubled. Each injection comprised three eggs, three and one-half ounces of milk, two ounces of liquid peptonoids, and half an ounce of whiskey. Through an inadvertence the tube escaped on the second day, but was replaced without difficulty or ill result. Twelve hours after the operation the patient began to be troubled with difficulty and an incessant desire to clear his throat, probably from the irritation of the contents of the esophageal pouch above the seat of stricture. This symptom yielded to frequent irrigations of the upper half of the esophagus with a 4 per cent. solution of boric acid and an infusion of coffee. The wound gave no trouble; the silkworm-gut sutures were removed on the thirteenth day. The patient sat up on the sixth day, and walked about on the fourteenth day. It was hoped to discard the tube, using it only for the purpose of making the nutritive injections.

Coincident Intra-uterine and Extra-uterine Fotation.—FRANKLIN (*British Medical Journal*, No. 1741, p. 1019) has reported the case of a woman, thirty-three years old, who had been in labor at term for eighteen hours,

with apparently some obstruction at the pelvic brim. She had already borne five children, and always with lingering labors. The woman appeared very ill and collapsed, with pale, sunken face and anxious aspect. The pulse was one hundred and fairly good. The abdomen was very prominent, especially above the umbilicus, and also tense; there was dulness on percussion in both flanks; palpation yielded no information. On vaginal examination, the os uteri could not be felt either by the fingers or by the hand. The vaginal wall was swollen and edematous. A hard mass, about the size of a coconut, could be felt exactly above, apparently pushing over the posterior vaginal wall, so as almost to give the impression that it was the anterior wall. This mass was evidently between the finger and the os uteri, and had lifted the cervix beyond reach. It was slightly movable and elastic and gave the impression of being a large sarcomatous growth filling up the pelvis. Rectal examination afforded no additional information. The bowels had been freely open; there had been trouble for some time in micturition. Upon consultation it was decided to open the abdomen immediately, and an incision six inches long was made from a point about half an inch below the umbilicus. About two ounces of blood at once escaped from the peritoneal cavity. The uterus presented, and the placenta was seen to be attached to its anterior wall. An india-rubber ligature was slipped over the fundus and brought gradually into the pelvis below the uterus and appendages. An incision about six inches long was made into the uterine wall; a rush of blood followed. A full-term, living, male child was easily extracted, together with the placenta. The ligature being now tightened, the hemorrhage at once ceased. A stout wire *écraseur* was substituted for the india-rubber ligature, and during its application there was some more bleeding. The uterus and appendages were now removed. For safety, a stout silk ligature was also passed around the stump, in case the *écraseur* should slip. From the pelvis there projected a huge swelling from which the hemorrhage was rather free. On manipulation the tightened capsule of this supposed growth ruptured, and a dead, full-term fetus was liberated, the vertex being in Douglas' pouch behind the uterine stump. The fetus was rapidly removed, and there ensued a terrific and uncontrollable hemorrhage from tufts of placenta attached in Douglas' pouch and to the cecum and the intestines. The bleeding did not cease until every vestige of placenta was removed. The patient lived only half an hour after the operation, although transfusion was tried.

Scorbutus in Infants.—From personal observation and a study of recorded cases, NORTHROP and CRANDALL (*New York Medical Journal*, No. 808, p. 641) conclude that scurvy may appear at any period of infancy or early childhood, although the disease is most common between the ninth and fourteenth months. The lesions are essentially hemorrhagic in character, probably as a result of diapedesis. The most characteristic are subperiosteal hemorrhages. Hemorrhages into the muscular tissues, into the skin, and into the mucous membranes, are also more or less constant. The disease occurs in every grade of the social scale, but is more frequent among the rich than among the poor. The neglected

child that eats everything at the table may become rachitic or marasmic, but he obtains enough fresh food to protect him from scurvy. The disease rarely occurs in asylums or hospitals, because of care in feeding. Lack of fresh food is its most important cause. The use of proprietary foods and condensed milk produces more scurvy in young children than all other causes. Even fresh milk in small proportions is not sufficient to insure protection. Anemia and malnutrition are almost invariably present; a peculiar, sallow complexion is common. Scurvy is frequently superadded to rachitis, but in a considerable number of cases no evidences of rachitis are present. So-called acute rickets is, in most cases, probably in all, rickets complicated by scurvy. Pain is a constant symptom of scurvy in children. It develops early, and is usually pronounced. A varying degree of immobility of the extremities is common, and is frequently so marked as to simulate paralysis. This pseudo-paralysis disappears with the subsidence of the scorbutic symptoms. Subcutaneous hemorrhages, as well as hemorrhages from the cavities of the body, are very common, but are not necessary to a diagnosis of scurvy. The condition of the gums is characteristic. They are purplish, soft, spongy, and bleeding, and frequently show pronounced ulceration. When the teeth have not appeared the changes in the gums are usually slight, or entirely absent. Painful swelling of the lower extremities is the most common symptom; the upper extremities are rarely involved. The thigh is more frequently affected than any other part. Untreated scurvy is a very fatal disease. Recognized and properly treated, a rapid and complete cure is usually effected. The result of anti-scorbutic treatment is in fact one of the most certain means of diagnosis. Scurvy may be mistaken for rheumatism, stomatitis, rickets, sarcoma, osteitis, and infantile paralysis. The disease is of dietetic origin, and must be cured by dietetic treatment. Fresh milk, beef-juice, and orange-juice are the most effective remedies.

Paralysis following Diphtheria of the Genitals.—GAYTON (*Lancet*, No. 3691, p. 1301) has reported the case of a well-developed and well-nourished girl, four years old, who presented excoriation of the groins, labia, and vulva, in consequence of an offensive, copious vaginal discharge. The vulva and the vagina were greatly swollen and covered with a diphtheric exudation. No urine had been passed for two days and the bladder-dulness extended a considerable distance above the pubes. As exquisite tenderness was present, chloroform was administered and with a catheter a small amount of albuminous urine was withdrawn. The temperature was 96°, the countenance pallid, and the pulse feeble and slow. The throat had at no time been affected. Recovery duly followed frequent applications of a solution of mercuric chlorid, conjoined with the administration of iron and brandy and a generous diet. The albuminuria, however, persisted, and the pulse and temperature remained subnormal. In the course of a month marked strabismus was observed, together with paralysis of the palate and regurgitation of fluids through the nostrils and an irritating, noiseless cough. Later, visual accommodation was impaired and the paralysis became more universal, the typical apathetic look and bearing being very pronounced. The muscles of de-

glutition became involved. Much headache was complained of. Feeding by the mouth had finally to be resorted to, but death took place, apparently from the action of the poison on the bulbar centers.

Rapid Retrocession of Sarcomata after Induced Labor.—JAHR (*Centralblatt für Gynäkologie*, 1894, No. 23, p. 545) has reported the case of a woman, thirty-six years old, in the seventh month of her eighth pregnancy, of cachectic appearance, who presented a hard nodule, as large as a walnut, in the right breast, a tumor as large as a hen's egg in the right axilla, and one as large as a hazelnut above the right clavicle. Upon the pectoral muscle was an enlarged gland. Two nodules, as large as hazelnuts were present at the introitus of the vagina, two on the anterior wall of the vagina behind the introitus, and two also on the anterior lip of the cervix. Other nodules, forming a collection as large as a hen's egg, could be felt in Douglas' pouch. Frequently recurring attacks of severe abdominal pain caused the patient much distress and led to the belief that the peritoneum was the seat of metastatic growths. It was decided to induce premature labor, and this was effected by the introduction of a small amount of glycerin between the membranes and the uterus. A partially macerated, dead child was delivered, and the secundines came away without delay. Soon after the labor the accessible tumors were observed to be smaller in size, some almost entirely disappearing. The woman remained weak, and without much change in her condition death took place in collapse six weeks after the delivery. Sections of portions of several of the tumors disclosed the histologic structure of sarcoma.

Reflex Spasm of the Tongue, Lips, and Pharynx Induced by Irritation of the Great Occipital Nerve.—GALLERANI and PACINOTTI (*Neurol. Centralbl.*, 1893, No. 14; *Centralbl. f. d. med. Wiss.*, 1894, No. 19, p. 335) has reported the case of a man who had been struck upon the head twelve years previously. The site of injury was from time to time the seat of pain, both spontaneous and induced. In the stellate cicatrix a small, round, hard body could be felt, pressure upon which induced pain. There was present contracture of the muscles of the left side of the neck, so that the head was held downward and directed to the left, and there was also disturbance of speech, contraction of the lips, slight trismus, and some difficulty in swallowing. This spasmodic condition was a result of irritation induced by the presence of the foreign body, and disappeared after excision of the cicatrix at the point of union of the great and small occipital nerves.

The Seat of the Formation of Urea in the Animal Organism.—As the result of personal investigation and from existing data, KAUFMANN (*Archives de Physiologie normale et pathologique*, 1894, No. 3, p. 531) reaches the conclusion that urea exists in all of the tissues of the mammalian organism and in greater proportion than in the blood. All of the tissues seem to take part in the production of urea, although in varying degree. The liver is the most active seat of urea-formation. The production of urea seems due to processes of denutrition that take place in various tissues, and results especially from the work performed by the liver.

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THE NECESSITY OF BACTERIOLOGIC EXAMINATION IN MEMBRANOUS THROAT-AFFECTIONS.

As yet the most practical results of bacteriologic research have been in the direction of the prevention of disease. Perhaps the greatest good has been accomplished in the case of Asiatic cholera and typhoid fever, but very much has been done in the prevention of all of the infectious processes.

During the past four years bacteriologic studies have added much to our knowledge of diphtheria. That the Klebs-Loeffler bacillus is the cause of this disease may now be regarded as established beyond doubt. It has also been shown that most cases of so-called membranous croup and membranous rhinitis are caused by the same organism. Anatomic lesions identical in appearance with those encountered in diphtheria may also be produced by other microorganisms—notably streptococci and other cocci—so that the diagnosis is often not to be positively made without bacteriologic examination. But it is the so-called pseudo-diphtheria-bacillus that assumes the greatest importance. This organism is found not only in cases presenting a small amount of false membrane, and likely to be diagnosed as follicular tonsillitis, herpetic pharyngitis, etc., but also in other cases with a more extensive formation of false membrane.

When this subject first began to attract attention it was claimed by a few that the pseudo-diphtheria-bacillus was constantly present in the pharynx—a condition of affairs comparable to the presence of the bacillus coli communis in the intestine and its relation to the typhoid-bacillus of EBERTH. This extreme view is now no longer held. But the relation of the pseudo-diphtheria-bacillus to the true diphtheria-bacillus has not been so clearly determined. It is claimed by ESCHERICH and many others that the former is a separate and distinct organism, but the majority of investigators in this line of research now look upon the so-called pseudo-bacillus as only an attenuated form of the true diphtheria-bacillus, identical morphologically with the true bacillus, and only distinguishable from this by the inoculation of susceptible animals, in which case it fails to kill.

If, then, this pseudo-bacillus be only the bacillus diphtheriae shorn of its virulence, the important question arises: Can this organism take on pathogenic properties again if subjected to certain conditions or sown upon proper soil? A definite answer to this cannot be given at the present time. Many bacteriologists have succeeded in diminishing the virulence of the true bacillus, so that it seems proved that the so-called pseudo-bacillus may be derived from the virulent form by placing it under conditions unfavorable for its development, such, for instance, as exposure to light. It is not so well established, however, that the virulent form may be derived from the pseudo-bacillus, although ROUX and YERSIN have augmented the virulence of the latter by inoculating guinea-pigs with it and at the same time with a streptococcus. Although neither microorganism alone could produce death, the two in conjunction proved fatal. Other microorganisms whose pathogenic properties have been lost by culture outside of the body may be restored to virulence by being passed through the body of susceptible animals. Until this question is definitely settled all cases with membrane in which the Klebs-Loeffler bacillus is found should be isolated. Observing clinicians have noticed that unquestionable diphtheria not uncommonly makes its appearance in families in which other members have been suffering with what was supposed to have been follicular tonsillitis, herpetic pharyngitis, or simple angina.

Recently quite a number of cases of sore-throat in which no membrane could be discovered have been investigated by different observers, who have

found virulent Klebs-Loeffler bacilli present, and there can be no doubt that these cases, though they themselves never present the clinical picture of diphtheria, may transmit the disease to susceptible persons. Such cases are, of course, more common in those constantly exposed to diphtheria.

Another important fact that has only recently been given prominence is that the true bacillus diphtheriae or the pseudo-diphtheria-bacillus may be present in association with other acute infections. It has been thought that different infections could not exist in an individual at the same time, and that the throat-conditions of such diseases as scarlet fever, typhoid fever, and measles were not produced by the same microorganism as is known to cause diphtheria.

In the great majority of cases of scarlet fever the sore-throat is caused by cocci, either streptococci or staphylococci, alone or associated, and yet in quite a large number of cases the bacillus diphtheriae is present, as a rule in association with cocci, but in exceptional cases alone. The proportion of such cases is ordinarily not large, although MORSE found the Klebs Loeffler bacillus present in the throat in twenty-three of ninety-nine cases of scarlet fever examined in the Boston City Hospital.

The sore-throat of measles is usually caused by cocci, and yet the Klebs-Loeffler bacillus has frequently been found present; while in typhoid fever complicated by the presence of false membrane in the pharynx this microorganism has been found present in a very large proportion of the cases examined bacteriologically. As COUNCILMAN has stated, perhaps pseudo-membranous inflammations of the throat in the course of typhoid fever are frequently overlooked because of the hebetude of the patient.

In follicular tonsillitis quite a variety of bacteria have been found, but there can be no doubt that diphtheria frequently coexists with this affection.

Of course, in all of these states the throat-condition may not have been primarily caused by the Klebs-Loeffler bacillus, but the soil being rendered favorable, secondary infection has occurred. At any rate, as KOPLIK concludes, *we should regard all specks on the tonsils as suspicious, and if not diphtheric, at least liable to become so.*

When the wide distribution of the bacillus diphtheriae, in either its virulent or its attenuated form, is considered, as well as the impossibility in many cases of determining clinically the nature of membranous throat-affections, the importance of system-

atic bacteriologic examination in all doubtful or suspicious cases must be obvious.

The example of the Health-Board of New York City in providing for bacteriologic examination in all suspicious cases were well followed by all large cities. Sterilized tubes containing cotton swabs can be obtained by any practising physician from any of numerous depots distributed over the city. With this material blood-serum tubes are inoculated, and in twenty-four hours a positive conclusion can be reached as to the presence or absence of the Klebs-Loeffler bacillus. Of the first 2000 cases examined in this way by the New York Health-Board 1442 were instances of true diphtheria, while 433 were examples of pseudo-diphtheria.

These figures indicate the amount of good to be realized from such a plan of systematic examination. It is perhaps no exaggeration to say that far more harm comes from the spread of diphtheria through mild cases, either not recognized or not reported and not isolated, than from the very malignant cases, for of these latter there is a popular fear, and they are summarily dealt with. It would, therefore, appear that health-boards could do far more good by instituting some means by which an accurate diagnosis could be made at once, and suitable measures in accordance therewith promptly adopted, than by detailing one or more men to watch a house harboring a malignant case of diphtheria, or by sprinkling carbolic acid in the back-yard and cellar of a house in which a case of ordinary severity develops.

Yet another point demands attention, viz., the time, after all membrane disappears, at which isolation may cease. In the light of recent investigations there is perhaps much harm done in this direction, for the bacillus diphtheriae has been found in the pharynx and nasal cavity in cases of diphtheria a much longer time after the membrane has disappeared than isolation is usually enforced. In a series of cases examination would perhaps show that the Klebs-Loeffler bacilli are present in the nasopharynx on an average from ten to fifteen days after the membrane has disappeared, but it must be remembered that in individual cases they may persist for five weeks, and in a case mentioned by WILLIAMS they were present eight weeks after the disease had come to an end. The bacilli found after the disappearance of the membrane are not the pseudo-bacilli, but possess active virulence, as has been shown by the inoculation of guinea-pigs. The length of time that these bacilli persist after an attack of diphtheria

seems to bear no relation to the severity of the disease, but it does seem to be longer in cases in which there is some chronic disturbance in the nasopharynx, while it is shorter in cases in which antiseptic sprays are long employed.

Isolation is certainly very often not continued long enough in private practice, while it has been found by TOBIESON that of a series of forty-six cases discharged from a hospital as cured, culture showed the presence of the Klebs-Loeffler bacillus in twenty-four.

The rule adopted by the New York Health-Board could be followed with advantage by other boards, *i. e.*, that no case can be considered free from the danger of spreading the contagion until culture has demonstrated the absence of the Klebs-Loeffler bacilli.

EDITORIAL COMMENT.

Facilis est Descensus.—A correspondent has called our attention to a reprint from the *Journal of the American Medical Association* upon "The Treatment of Typhoid Fever," accompanied by a circular-letter, with both of which we have also been favored. If our readers will recall the circumstances under which the original publication of the article reprinted was made, the criticism it then elicited, the extraordinary journalistic departure and the ordinary abusiveness with which that criticism was met by *The Journal of the American Medical Association*, and the fact that the author was at San Francisco deemed worthy to be elected one of the trustees of THE AMERICAN MEDICAL ASSOCIATION; if they will then for a moment consider the attitude of the said trustees toward outright nostrum-advertising, they will see that on the matter under consideration, which can hardly be designated one of outright nostrum-advertising, comment on our part would be as useless as it is superfluous. We, therefore, simply cite the title of the reprint and reproduce the circular with which we have been favored, leaving the members of the profession to form their own judgment.

"Can Typhoid Fever be Aborted? Read before the Mississippi Valley Medical Association, October, 1893. By J. E. Woodbridge, M.D., Youngstown, O. Reprinted from *The Journal of the American Medical Association*, February 10, 1894. Chicago: Printed at the office of the *Journal of the Association*, 1894.

"MY DEAR DOCTOR: I have made especial arrangements for the reception at the Youngstown City Hospital of all cases of typhoid and malarial fever, where, if sent for my treatment, they will receive the best possible care and nursing; and, as by the treatment I have advised there is never any danger of intestinal lesions, there can be no objection to moving these cases any distance.

"May I invite your attention to the enclosed reprint of the first part of my paper, written for the Mississippi Valley Medical Association, the second part of which, giving treatment, will be sent if you wish it.

"Fraternally yours,

"JOHN ELIOT WOODBRIDGE."

"YOUNGSTOWN, O., August 13, 1894."

REVIEWS.

A TEXT-BOOK ON DISEASES OF THE EYE. By HENRY D. NOYES, A.M., M.D. Second revised edition; 816 pages, illustrated. New York: William Wood & Company, 1894.

CONCERNING this second edition the author says: "In revising this treatise numerous additions have been made to render it more complete. For example, those parts of cerebral anatomy and pathology which have a bearing upon the eye have been discussed at unusual length for such a work; this is true both for the motor nerves and for the optic nerves. Some chapters have been considerably modified and some have been rewritten; for example, those on granular conjunctivitis, asthenopia, sympathetic ophthalmia, glaucoma, cataract, hemianopsia, etc."

The selection of thin paper permits a desired reduction in the size of this volume. The illustrations on the whole are good, but the proof-reader has been careless at times, as for instance in "*atrophia*," p. 68; plate III, instead of A, p. 651.

Exceptions may be taken to certain statements in the chapter on refraction: Gr. x of homatropin to $\frac{3}{4}$ J, instead of gr. xx, have in conjunction with cocaine been found a very competent substitute for atropin by so many skilful ophthalmologists that the indications for its use should have passed the line of uncertainty in a new edition of a text-book. Likewise will a careful study of many thousand cases convince the investigator that perhaps one-half, if not more, have refractive errors less than 1 D., especially in astigmatism, and if the ophthalmometer and ophthalmoscope can claim only an accuracy of 1 D. within the true state of refraction, the question naturally arises: Why use these instruments if you have to rely, after all, upon the test-glasses? And if so, why not say so?

Careful refractionists will also disagree with the author's statement that the use of a mydriatic is rarely necessary in myopic astigmatism, but will insist that it should be employed in all refraction-work, at least up to the age of presbyopia, if good results are to be obtained.

It hardly becomes an author of Dr. Noyes' standing to ignore the much-discussed and often-contradicted question of "graduated tenotomy" with the simple statement of "I have had no experience with it." The author of a text-book should not pass over one of the leading questions of the day in such an indefinite manner, but should at least present the arguments and facts in such a way that the student can form some kind of opinion about the usefulness of the operation, and not beg the question by quietly ignoring it.

Objections will also be made by many to the advice as to wearing prisms constantly for moderate degrees of muscular error. While probably few will deny their usefulness, it should not be forgotten that they rarely, if ever, cure the existing evil, while gymnastic prism-exercises not only do away with the wearing of the prisms, but positively, and, as far as experience can demonstrate, *permanently cure* not only moderate, but quite large insufficiencies of the interni.

The chapters on diseases of and operations on the eye are written and illustrated in a clear and concise

manner, and the author's large experience shows all through this portion of the work.

A well-selected bibliography of useful literature on the eye and a carefully compiled index close a book which will prove not only an excellent text-book for students of ophthalmology but also a convenient reference-book for the general physician.

BIOGRAPHY OF EMINENT AMERICAN PHYSICIANS AND SURGEONS. With Many Portraits. Edited by R. FRENCH STONE, M.D. Indianapolis: Carlin & Hollenbeck, 1894.

THIS is a work involving great expenditure of time, money, and industry. It consists of biographic sketches of living and dead physicians, in many cases illustrated with portraits, and in some cases with autographs. There is a general index, and a classified index under city names, giving the residence and line of practice of the living among the subjects of the sketches. How volumes of this kind are prepared, and what is the *quid pro quo*, not in the case of all, but in the case of 90 per cent., of the persons mentioned, is an open secret. The opinions of good men differ as to whether or not the practice is worthy. We have been interested in comparing the teaching list of Philadelphia colleges with the index of this volume, and the suggestiveness of the omissions of names of dead and living teachers thus discovered is very great. We give a partial list herewith: A. C. Abbott, Roberts Bartholow, J. Solis-Cohen, John C. Da Costa, W. S. Forbes, S. W. Gross, F. P. Henry, Henry Leffmann, Clara Marshall, William F. Norris, Charles A. Oliver, William H. Parish, John B. Roberts, J. H. C. Simes, J. Madison Taylor, James B. Walker.

The average of eminence of these teachers and authors is certainly not below that of the average "eminent physician and surgeon" mentioned in the work. It is an entertaining matter to distinguish between the articles in preparing which the subjects have had no part and those which are autobiographic in origin if not in form. Among the latter it is also interesting to observe the relative extent to which different men have "written themselves up." Thus the editor disposes of Dr. Osler in some twenty lines, while many a man whose name is unknown to us, and probably to nine-tenths of the medical world, takes two columns or more to describe in glowing terms his personal qualities, enlarge upon his wife's pedigree, give long lists of the distinguished professors and tutors whose instruction he has enjoyed, and catalogue every insignificant paper he has read at society meetings or inflicted on the journal-reading public.

Notwithstanding all this, the book contains much valuable and interesting information, and we trust the sale will be sufficiently great to compensate the editor for the time, labor, and money he has expended in its preparation.

A SYSTEM OF LEGAL MEDICINE. By ALLAN McLANE HAMILTON, M.D., and LAWRENCE GODKIN, Esq., assisted by numerous Collaborators. Vol. I, 657 pages. New York: E. B. Treat & Co., 1894.

It is apparently nowhere definitely stated in this volume as to how many volumes this system is to comprise,

but even if only an additional one is to be issued the work will be an extended manual of medical jurisprudence. There will soon be no lack of guidance in this field, for several "systems" are in course of publication. In the present work American contributors are exclusively engaged, except that incidental to the chapter on "Death in its Medico-legal Aspects" Dr. F. A. Harris has availed himself of the notes by a London physician of examination of mutilated remains found in various parts of that city. These are noted in the preface as the Whitechapel cases, but with one exception they do not seem to be the cases that made "Jack the Ripper" notorious some years ago. There is, indeed, in modern treatises on medical jurisprudence a tendency to excessive multiplication of illustrative cases and to full descriptions of them. This method makes interesting reading, for the records of this department are highly dramatic, but the tendency ought to be curbed.

The present volume contains numerous woodcuts and several color-plates. Among the latter is to be especially noted the frontispiece, a representation of the mucous surface of the stomach of a child poisoned by arsenious oxid. The plate representing the color-tests of some of the common alkaloids is an interesting example of perverted ingenuity. These colors can be easily obtained for comparison and study by the chemist, and why the publisher should be put to the expense of such unnecessary illustration is past guessing. Plates of color-reactions of rare bodies or of typical appearances that cannot be produced at will are, of course, valuable, but the intelligent chemist who wants to study the color-reactions of strychnin will do so with the materials themselves and not by examining these plates.

Among the specially interesting articles are those on "Ptomain" and on "Indecent Assault on Children." The former article is contributed by Dr. Vaughan, and the latter by Dr. Gibbs, examining physician to the New York Society for the Prevention of Cruelty to Children.

The illustrative cases in all parts of the volume are largely American, and serve to show how rapidly the larger cities in this country are approaching the degraded moral and physical state of the European capitals.

The typography is good, and there is a commendable absence of italics, displayed titles, and confusing subdivisions.

TRATATO ELEMENTAL Y PRACTICO DE PATOLOGIA Y CIRUGIA GINECOLOGICAS. Por M. M. PONTE. Primero volumen. 8vo, pp. 475. Curazoa: A. Bethencourt & Son, 1891.

GYNECOLOGIC works in Spanish are a rarity, and this one from the pen of a Caracas practitioner, engaged as a specialist, is a novelty from Central America, which as yet has contributed but little to medical science. Dr. Ponte dedicates his work to Dr. Jules Péan, of Paris, well known in the field of gynecologic surgery, and according to his preface, must have completed his first volume in August, 1891, or nearly three years ago. His bibliographic list includes 201 names of authors consulted in preparing the volume, many of them well-known writers in French, German, Italian and English; among the last being the six Americans, Battey, Bozeman, Emmet, T. G. Thomas, Goodell, and Levis. Several Caracas physicians have been educated in Phila-

delphia; but among the young men of Spanish America, French is so generally learned that they naturally go to France for a medical education and degree, and are therefore most familiar with French medical ideas and progress. Cuban young men quite commonly learn both French and English, and hence are likely to study in the United States, where there are many from their own country engaged in business; but men of Spanish blood are not given to writing medical books, although they do contribute to their own journals; Spain is much behind Italy in original medical thought and practice.

This work of Dr. Ponte was prepared after a gynecologic experience of twenty-five years. It is purely elementary; treats of a large number of female diseases systematically and briefly, and presents no illustrations of any kind. Two hundred and fifty-nine pages are devoted to a study of the vulva and its diseases, including defects of development, hermaphroditism and dermatology; these constitute Part I. Part II treats of the vagina, and covers sixty pages; and Part III of the urethra, in fifty-five pages. The first part treats of forty-eight diseases and their varieties, located in the vulva. These are not illustrated by cases, and we are left in the dark as to their respective prevalence in Venezuela. This volume is mainly a compilation, and its author lays no claim to originality; the second may be of quite a different character.

CLIMATES OF THE UNITED STATES: IN COLORS. By CHARLES DENISON, A.M., M.D. Being a popular edition of Denison's Charts. Chicago: The W. T. Keener Company, 1893.

LIKE many tables of medical statistics, the stereotyped meteorologic records which are invoked and quoted by the interested advocates of every sort of climate under the sun are usually worse than worthless; they are delusive. Actual climates refuse to be mapped and labelled. These conventional weather-records are looked upon by travel-wise people much as the veteran practitioner looks upon the crisp diploma of the just-hatched graduate: "From this document, young man, I assume that you intend to become a doctor."

An intelligent digest of the facts supplied by the Weather Bureau furnishes a plausible basis from which to deduce the leading characteristics of a given climate, even when actual residence discloses specially favorable or unfavorable characteristics which are utterly inexplicable from the recorded data. Climatography may, therefore, be said to bear about the same relation to the real characteristics of any locality that geography bears to the actual, verdure-clad landscapes and esthetic perspective of a country. Nevertheless, weather-records are as indispensable to the student of climates as maps to the traveller; and while Dr. Denison does not claim to have done as much for his pet subject as Mercator did for geography, he has added considerably to our facilities for studying and comparing local climatic conditions in the United States, and deserves the thanks not only of the profession but of the community, for arranging and presenting in a readily accessible form such salient facts concerning the climatology of this country as can be embodied in diagrams, colored charts, and apt compilations of accumulating statistics.

The work is issued in a style both convenient for the reader and creditable to the publishers.

CONGENITAL AFFECTIONS OF THE HEART. By GEORGE CARPENTER, M.D. Lond. 8vo, pp. 103. London: John Bale & Sons, 1894.

FOR a proper conception of congenital affections of the heart, a knowledge of the various phases of development through which this viscus and its great vascular trunks pass is primarily essential, as many of such lesions are due directly or secondarily to abnormalities of development. It would be interesting (if the point could be determined) to know in what proportion of cases, if in any, defects of development occur independently of palpable exciting causes, such as inflammatory conditions, mechanical influences, traumatism, and the like. This thought is suggested by a perusal of the little book before us, in which is described the mechanism of the various morbid conditions of the heart and great vessels found at birth. The etiologic factors are briefly discussed, the symptomatology at greater length; the diagnosis, with its difficulties, is duly considered; the factors upon which the prognosis is based are indicated; the measures to be adopted in treatment are pointed out. The work, while not elaborate and discursive, presents in a simple manner the essential facts concerning a somewhat neglected subject.

THE CARE AND FEEDING OF CHILDREN. By L. EMMETT HOLT, M.D., Professor of Diseases of Children in the New York Polyclinic; Attending Physician to the Babies' Hospital and the Nursery and Child's Hospital, New York. New York: D. Appleton & Co., 1894.

THIS neatly-bound and well-printed little book of sixty-two pages has been written for the use of mothers and nurses who have the care of children. The question-and-answer method has been followed, and everything has been sacrificed to clearness and simplicity. The book contains such knowledge as should be acquired by every nurse and mother, and is admirably adapted to fill the place for which it is intended.

WEEKLY ABSTRACTS OF SANITARY REPORTS ISSUED BY THE SUPERVISING SURGEON-GENERAL, M.-H. S. Vol. VIII, Nos. 1 to 52. Washington: Government Printing-office.

THIS volume of thirteen hundred pages and index contains a large amount of statistics and other materials of sanitary interest, issued in weekly bulletins during the year 1893. It is impossible to give a summary of its contents, but we may note the text of the U. S. Quarantine Act, accounts of methods of disinfection and inspection at various foreign and domestic ports, and tables of mortality in the principal cities of the world. A feature of interest is the low death-rate from enteric fever in most of the cities of Europe.

ESSENTIALS OF PHYSICS. By FREDERICK J. BROCKAWAY, M.D. Second edition. Pp. 318 and index. Philadelphia: W. B. Saunders.

THIS book is arranged on the question-and-answer system, though it seems from the brevity of the questions

and the fulness of the answers that the author is not much of a believer in the plan. There can be but little doubt that whatever disadvantages attach to condensed manuals for the use of students, the system of questions and answers is the most objectionable method. In this manual the questions amount to little more than substitutes for side-headings, and the answers frequently extend into accessory fields.

The present edition has been prepared with the assistance of some recently-issued works on physics. It is a good summary of this department of science.

VITAL STATISTICS OF THE DISTRICT OF COLUMBIA AND BALTIMORE, COVERING A PERIOD OF SIX YEARS, ENDING MAY 31, 1890. BULLETIN OF CENSUS OFFICE. BY JOHN S. BILLINGS, M.D. Government Printing-office.

ALTHOUGH consisting largely of mortality-tables, this bulletin deserves notice, because it presents a careful study of the distribution of causes of death in Baltimore and Washington by sanitary districts, a method that is much superior to classification by wards. Wards have usually merely political boundaries and ought to be disregarded in sanitary reports. Many maps of uniform scale are introduced, showing proportional prevalence of important diseases.

CORRESPONDENCE.

THE MEETINGS OF THE COLLEGE OF PHYSICIANS.

To the Editor of THE MEDICAL NEWS,

SIR: Noticing in THE MEDICAL NEWS a list of society-meetings, in which those of the College of Physicians are mentioned, I beg to state for the information of the readers of THE NEWS the conditions upon which visitors are received at meetings of the College or of its Sections.

1. Meetings of the College. "At the stated meetings, except during the consideration of the private business of the College, strangers may be present on the invitation of a Fellow, who shall present them to the President and shall be held responsible for the character and professional standing of the persons introduced by him."—*Ordinances and By-laws, Chapter IV, Section 4.*

2. Meetings of Sections. "No person not a Fellow of the College shall attend any meeting of a Section except by specific invitation to that meeting by a Fellow, who shall give the name of the visitor to the Clerk of the Section and be responsible for the visitor's actions at the meeting. No person not a Fellow shall take part in any discussion at a Section meeting, except on invitation of the President or Chairman."—*Standing Rules for Sections, Section 6.*

Yours respectfully,

CHARLES W. DULLES, Secretary.

NEWS ITEMS.

The British Medical Association will hold its sixty-third annual meeting in London July 30 and 31 and August 1 and 2, 1895, under the presidency of Dr. Russell Reynolds.

Cholera.—It is reported that twenty-one cases of cholera occurred at Marseilles between August 4th and 20th. About two hundred fresh cases of cholera are reported daily in Galicia. The mortality exceeds fifty per cent.

A Pasteur Institute in Vienna has been established for the treatment of cases of hydrophobia by the method of inoculation of Pasteur.

BOOKS AND PAMPHLETS RECEIVED.

Enterorrhaphy; its History, Technique, and Present Status. By N. Senn, M.D., Ph.D., LL.D. Reprinted from the Journal of the American Medical Association, 1893.

A Manual of Minor Surgery and Bandaging, for the Use of House-Surgeons, Dressers, and Junior Practitioners. By Christopher Heath, F.R.C.S. Tenth edition. Philadelphia: P. Blakiston, Son & Co., 1894.

A Case of Suprapubic Cystotomy in which the Bladder was Distended with Air Instead of Water, and Four Hundred and Ninety-five Calculi Removed. By W. W. Keen, M.D. Reprinted from the Journal of Surgery, Gynecology, and Obstetrics, 1893.

Relation between Diseases of the Upper Air-passages and Diseases of the Eye. By J. G. Carpenter, M.D. Reprinted from the University Medical Magazine, 1893.

The Mechanical Treatment of Osteitis of the Knee. By Henry Ling Taylor, M.D. Reprinted from the New York Medical Journal, 1893.

Fourth Annual Report of the College Settlements Association. From September 1, 1892, to September 1, 1893. Philadelphia: Avil Printing and Lithographing Co., 1894.

A Study of Dietaries. Partial Report of Dutton Fellow, College Settlements Association, 1892-93. By Amelia Shapleigh. Pamphlet.

The Subjective Value of a Social Settlement. By Jane Addams. Reprinted from the Forum, for November, 1892.

A Contribution to Our Knowledge of Epidemic Cerebro-spinal Meningitis. By Simon Flexner, M.D., and Lewellys F. Barker, M.B. Reprinted from the American Journal of the Medical Sciences, 1894.

Gangrene of the Scrotum. By Charles W. Allen, M.D. Reprinted from the Journal of Cutaneous and Genito-Urinary Diseases, 1894.

Opium as an Hypnotic in Old Age. By J. Y. Dale, M.D. Reprinted from the University Medical Magazine, 1894.

The Annual Report of the Superintendent and Physician of Walnut Lodge Hospital, Hartford, Conn., at the Annual Meeting, January 3, 1894. Hartford, Conn.: Press of Case, Lockwood & Brainard Co., 1894.

Total Extirpation of the Uterus. Cases Illustrating Various Indications for and Different Methods of Performing the Operation. Remarks upon Removal of the Uterus in Diseases of the Appendages. By George M. Edebohls, A.M., M.D. Reprinted from the Transactions of the New York Obstetrical Society, 1892.

The Technique of Total Extirpation of the Fibromatous Uterus. By George M. Edebohls, A.M., M.D. Reprinted from the American Journal of Obstetrics, 1893.

Addresses by the President, Samuel C. Busey, M.D., at the Celebration of the Seventy-fifth Anniversary of the Medical Society of Washington, D. C., and at the Banquet, February 16, 1894. Pamphlet. Washington, D. C.: Gibson Brothers, 1884.

Twenty-sixth Annual Report of the New York Orthopedic Dispensary and Hospital, 1894.

Improved Apparatus for Pott's Disease of the Spine. By Henry Ling Taylor, M.D. Reprinted from the Canada Medical Record, 1893.

Infantile Scorbatus. By Henry Ling Taylor, M.D. Reprinted from the American Medico-Surgical Bulletin, 1894.